MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

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I.—THE ANALOGY OF FEELING

By Stuart Hampshire

1. I am concerned in this paper with only one source of
one of the many puzzles associated with our knowledge of
other minds. It is often said that statements about other
people's feelings and sensations cannot be justified as being
based upon inductive arguments of any ordinary pattern, that is,
as being inferences from the observed to the unobserved of a
familiar and accepted form; I shall argue that they can be so
justified. I will not deny that such inferences are difficult;
everyone has always known, apart altogether from philosophical
theory, that they are difficult; but I will deny that they are
logically peculiar or invalid, when considered simply as inductive
arguments.   I believe that modern philosophers have found some-
thing logically peculiar and problematical about our inferences
to other minds, and have even denied the possibility of such
inferences, at least in part because of an incomplete understanding
of the functions of pronouns and of other contextual expressions
in our language; in particular they have misunderstood the
proper use of these expressions in combination with words like
" know ", " certain ", " verify ", " evidence ". If I am right,
it becomes easier to explain why what the solipsist wants to say
cannot properly be said, why solipsism is a *linguistically* absurd
thesis, and at the same time to explain why it is a thesis which
tempts those who confuse epistemological distinctions with
logical distinctions.

2. For reasons which will become clear later, I shall introduce
two quasi-technical terms.   As specimens of the type of sentence,
the status of which, as normally used, is in dispute, I shall take

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the sentences, " I feel giddy ", " you feel giddy ", " he feels giddy",
and so on through the other cases of the verb " feel ". Any
normal use of the sentence " I feel giddy " will be, in my in-
vented terminology, a specimen of an autobiographical state-
ment, where this phrase is simply shorthand for " a statement
describing somebody's momentary feelings or sensations which is
expressed in the first person singular ". Any normal use of the
sentences " he feels giddy ", " you feel giddy ", or " they feel
giddy ", will be specimens of heterobiographical statements—
that is, statements describing somebody's feelings which are not
expressed in the first person singular ; " we feel giddy ", as
normally used, would be a statement which is partly auto-
biographical and partly heterobiographical in my sense. It
may sometimes happen that someone chooses to tell the story
of his own inner life, using not the first person singular, but
the third person, or some fictitious or other name ; it is actually
possible to write one's own obituary notice, using the third person
and including within it descriptions which are intended as des-
criptions of one's own feelings and sensations. But on such
occasions the pronouns (or verb-cases for an inflected language)
are misleadingly used, and deliberately so. The ordinary
function of the word " I " (or of the corresponding verb-case in
inflected languages) is to indicate explicitly that the author of the
statement is also the designated subject of the statement ; the
exceptional, deliberately misleading uses mentioned above
consciously take advantage of this fact. By " an autobio-
graphical statement " I shall mean a statement describing
someone's feelings or sensations which explicitly shows, in the
actual form of its expression, that the author of the statement
is also its designated subject. A statement, e.g. in a novel,
about which we can argue, by reference to evidence external
to the verbal form of the statement itself, whether it is, as a
matter of fact, an autobiographical statement, will not therefore
be an autobiographical statement in my artificial and restricted
sense.

It has often been noticed that there are certain peculiarities
about these first person singular statements about feelings and
sensations, particularly when the main verb is in the present
tense ; these peculiarities have led some philosophers to charac-
terise them as incorrigible statements and have led others to deny
them the title of " statement " altogether ; the peculiarities
emerge in the use of words like " know ", believe ", and " cer-
tain " in combination with these sentences, or rather in their lack
of use. In respect of most statements, " I think that P is

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true but I may be mistaken" and "I have established that
P is true beyond all reasonable doubt" are sentences having
a normal use, whatever P may be; but there are no normal
circumstances in which one would say "I think that I feel giddy
but I may be mistaken" or "I have established beyond reasonable
doubt that I feel giddy", and consequently there are no normal
circumstances in which it would be in place to say "I am ab-
solutely certain that I feel giddy". By contrast the sentences
"you feel giddy" or "he feels giddy" do normally occur in
statements of the form "I believe that he feels giddy but I am
not certain" or "it is known that he feels giddy", and so on;
but again, "he believes that he feels giddy" or "he is certain
that he feels giddy" have no normal use. It is the corollary of
this that the questions "how do you know?" or "what is your
evidence?" are out of place in respect of statements about
momentary feelings and sensations, when addressed to the author
of the statement, if he is also explicitly shown to be the desig-
nated subject of it.

One inference which might be drawn from these facts is that
heterobiographical statements about feelings can never be
known to be true directly, where "known directly" means
that no question arises of how the statement is known to be
true and no question arises of any evidence being required
to support the statement. But this, as it stands, would be
a plainly false conclusion, since the person who is the designated
subject of such a heterobiographical statement does generally
know directly, without need of evidence, whether the statement
made about him is true or false. The proper conclusion is only
that the *author* of a heterobiographical statement of this kind
can never know directly, in the sense indicated, whether the
statement he has made is true or false; the author can always
properly be asked how he knows, or on what grounds he believes,
his heterobiographical statement to be true; he is required to
produce his evidence. So the so-called asymmetry is not a matter
of statements expressed in the first person singular, *as such*,
being different in respect of the evidence which they require from
statements expressed in the second or third person singular;
both descriptions of feelings in the first person singular, and
those in the second and third person, may be challenged either
by reference to indirect evidence (*e.g.* "I am sure you are lying;
you have obvious motives for lying, and you show none of the
symptoms which usually go with feeling giddy") or by a proper
claim to direct knowledge (*e.g.* "I can tell you quite definitely
that I do feel giddy, in spite of the evidence to the contrary").

This point is obvious, but it is apt to be dangerously slurred over when philosophers talk in general of " statements about other minds ", and then go on to enquire into *the* methods appropriate to confirming or confuting such statements. They may be thought to mean by " statements about other minds " what I have called heterobiographical statements—that is, statements describing feelings and sensations which are not expressed in the first person singular ; but the so-called problems of other minds, which is sometimes presented as a problem of how a *certain kind of statement* can be tested, does not attach to *a class of statements of any one particular form ;* it arises equally for first person singular statements, if in this case the position of the audience is considered instead of that of the author. The problem of other minds is properly the problem of what tests and verifications are ever possible for anyone who is not in fact the designated subject of a statement about thoughts and feelings ; it arises equally for any statement about feelings, whether the statement begins with the word " I " or with the word " you " or " we " or " they ".

3. The commonsense answer to the question, so reformulated, seems obvious—indeed so obvious that simply to give it cannot possibly satisfy philosophers ; something more is required to explain why it has been thought inadequate. The commonsense answer is : each one of us is sometimes the designated subject of an autobiographical statement and sometimes the subject of heterobiographical statements ; each one of us sometimes makes, or is in a position to make, statements about feelings which are not inferential and do not require supporting evidence, and also makes, or is in a position to make, statements about feelings which are inferential and do require supporting evidence. All that is required for testing the validity of any method of factual inference is that each one of us should sometimes be in a position to confront the conclusions of the doubtful method of inference with what is known by him to be true independently of the method of inference in question. Each one of us is certainly in this position in respect of our common methods of inference about the feelings of persons other than ourselves, in virtue of the fact that each one of us is constantly able to compare the results of this type of inference with what he knows to be true directly and non-inferentially ; each one of us is in the position to make this testing comparison, whenever he is the designated subject of a statement about feelings and sensations. I, Hampshire, know by what sort of signs I may be misled in inferring Jones's and Smith's feelings, because I have implicitly noticed (though probably not formulated) where

Jones, Smith and others generally go wrong in inferring my feelings. We all as children learn by experiment how to conceal and deceive, to pose and suppress ; concurrently we are learning in this very process how to detect the poses and suppressions of others ; we learn the signs and occasions of concealment at first-hand, and we are constantly revising our canons of duplicity as our own direct experience of its forms and occasions widens.

These are the commonsense considerations which seem at first glance to allow us to regard any heterobiographical statement, made by any one of us, as the conclusion of a valid inductive inference, the reliability of the method of inference used in any particular case being in principle testable by each one of us in confrontation with direct experience, that is, with non-inferential knowledge about the successes and failures of this particular method ; and I think that, as is usual in these questions, the third glance will confirm the first. But before going further, it is worth noticing how the argument from analogy, as stated by philosophers, approaches what I have called the commonsense position, but also misrepresents and over-simplifies it. There is a sense of " analogy " in which it is true that I could justify my inference that Smith is now feeling giddy by an analogy between the particular method of inference which I am now using and other uses of the same methods of inference by other people in discussing my feelings and sensations ; I know by direct experience how such feelings as giddiness are concealed and revealed; both I and Smith have been in a position to test the reliability of those methods of indirect inference about giddiness and cognate sensations which we from time to time use in talking about other people. The argument from analogy, as commonly stated by philosophers, only fails because the analogy has been looked for in the wrong place. What is required is not some simple analogy between my feelings and my external symptoms on the one hand and someone else's external symptoms, and so someone else's giddiness-feeling, on the other ; what is needed, and is also available, is an *analogy between different uses of the same methods of argument by different people on different occasions*. The inductive argument, the reliability of which is to be tested by each one of us, attaches both to the sentence " I feel giddy " and to the sentences " you feel giddy ", " he feels giddy ", etc. ; it attaches to any sentence of the form " X feels giddy " ; anyone hearing or using any sentence of this form, and anyone needing to test the statement conveyed on a particular occasion, can find such confirmation by looking for an analogy with occasions of its use when he was not in

need of such inductive confirmation. To anyone entertaining a doubt about the justification of a particular method of inference about feelings and sensations, the reassuring analogy is between the different occasions of use of the sentence in question; for on some of these occasions the doubter, whoever he may be, was in a position to know non-inferentially that the method of inference now in question led to a correct or incorrect conclusion. Each of us is in a position to learn from his own experience that certain methods of inference to conclusions of the kind " X feels giddy " are generally successful. Of course, if I, Hampshire, have never felt giddy myself, or had any sensation which is even remotely like this one, I would to that extent be at a loss to know whether other people are speaking the truth when they describe autobiographically this utterly unknown kind of sensation. In certain extreme cases this total failure of testability, and there-fore failure of communication, does in fact happen; in such cases I am in fact content to admit that I personally have no means of knowing whether what is said by others is pure invention or not; I simply do not know what they are talking about. But over the normal range of statements about feelings and sensations of which I am either the author or audience, I can generally point to occasions on which I was the subject of the particular state-ment in question and other people had to use the now questionable method of inference. Suppose that Smith and I each suspect the other of deceiving and of encouraging the other to use unreliable methods of inference. This again is a testable and empirical doubt, because we each of us know how we ourselves proceed when we are trying to deceive in this particular manner. We each base our devices of deception on our observations of other people's methods of inference about us. We each know that there is something in common to our different methods of de-ception, since we each sometimes know that we have failed to deceive and so we each know from our own experience how such deception may be detected. But no common psycho-logical language could be established with beings, outwardly human and sensitive, who never tried openly and in words to infer our feelings and who never acknowledged in words our success in inferring theirs, using the one to guide them in the other in a circle of mutual correction. We would have no good inductive grounds for speculating about the feelings of utterly silent people, or of people who did not betray themselves in speculating about us. It is merely a matter of natural history, and not of logic, that total failures of communication and understanding do not occur more frequently, and that in fact we

are each generally in a position to reassure ourselves about our methods of inference to the feelings of others by confrontation with the successes and failures of others in talking about us.

It has been necessary first to insist on the truism that all statements about feelings and sensations, including such statements expressed in the first person singular, are "statements about other minds" for some people, but not "statements about other minds" for other people; for it is precisely this feature of them which allows any one of us to test in direct experience the reliability of the numerous specific methods of inference which he uses when talking about the feelings of others. The importance of the truism can be brought out in the analogous case of "statements about the past"; philosophers have sometimes invented perplexities by writing as if we could pick out a class of statements as "statements about the past" and could then enquire how such statements can possibly be established as true by inductive argument; for how—it is asked—can we ever in principle confirm the validity of our inferences about the past? The mistakes which lead to this question are the same as in the "other minds" case. We cannot pick out a class of statements as statements about the past, unless we mean merely statements expressed in the past tenses. But the tenses, like the pronouns and cases of verbs, serve (among other functions) to relate a statement to the particular context or occasion of its utterance or of its consideration; clearly the *same* statement may be a statement about past, present or future, when considered, accepted or rejected by different people in different contexts; similarly, the *same* statement may be made either heterobiographically or autobiographically. A statement in the present tense, which is in this artificial sense a statement about the present, when verified and reaffirmed, may be reaffirmed as a statement about the past, and equally a statement about the future, when finally confirmed, may be reaffirmed as a statement about the present. *The very notion of confirmation involves this possibility of comparing the different contexts of utterance of the same statement.* It does not in general lie *within* the statement itself, or in its grammatical form of expression, that it is a statement about the mind of another, or that it is a statement about the past; these are features of the circumstances of the utterance or consideration of the statement, features which are partially indicated (but not stated) by pronouns, by tenses and by other contextual expressions, whenever and by whomever the statement is asserted, re-asserted or denied. Strictly speaking, there can be no class of statements about the past, standing in hopeless

need of confirmation, any more than there can be a class of past events ; similarly there can be no class of minds which are other minds, or class of statements about them.   This confusion of contextual idioms such as " other ", " past " with class-terms has its roots in an unnoticed double use of the idioms, which must now be explained.

3. It is often suggested that the function of pronouns and other contextual expressions (" this ", " that", " here ", " now ", etc.) is to designate or to refer uniquely to some person, thing, time, place, event, etc.   Mr. Strawson (MIND, October, 1950) has suggested the appropriate label, " uniquely referring ex-pressions " ; certainly one of the ways in which pronouns and these other contextual expressions are used is in this uniquely referring way—that is, to indicate, in a particular context of utterance, a particular person, thing, event.   But it is character-istic of the contextual expressions that they are not always or solely used to refer uniquely or to designate a particular person, thing, event ; they also have an important *generalised* use, in which they make no reference to a particular individual and in which they can be interpreted without any reference whatever to any particular context of utterance.   Consider the slogan, " do it *now* " ; or " never put off till *to-morrow* what you can do *to-day* ".   In this use " now ", " to-day ", " to-morrow " do not refer uniquely, but have a force in some (but not all) ways like that of a variable, and might be expanded into " now, to whatever moment ' now ' may refer ", or " to-day, whatever day ' to-day ' may refer to ".   Another example : " the future is quite uncertain " : as it stands, and without a context, this sentence is ambiguous, and might be used to make two quite different statements, or even two different kinds of statement ; " the future " might be used in the uniquely referring way, so that we require to know the context of utterance in order to know what particular stretch of history is being referred to and described as uncertain ; or " the future " might be used in the purely generalised way—" the future, at whatever point in history, is always uncertain ".   This familiar generalised, or quasi-variable, use is transferred to philosophy when we talk of " statements about *the past* ", " statements about the *other* side of the moon ", and " statements about *other* minds ". Confusion between the two kinds of use arises when a transition is made within a single argument from the generalised to the uniquely referring use, or vice versa, without this transition being noticed ; and just this is what generally happens in arguments about our knowledge of " other minds " and in formulations of the

so-called *ego*centric predicament. The solipistic doubter will probably not put his question in the explicitly generalised form, but will ask : " How can *I* ever justify my inferences about what is going on in *your* mind, since *I* can have no independent means of checking *my* inferences about *your* feelings ? " There may be a muddle in this : Does the " I " here mean " I, Hampshire " ? Is it a lament about my, Hampshire's, peculiar isolation and the peculiar inscrutability of you, Smith ? Or does the " I " mean " whoever ' I ' refers to ? " and the " you " " you, whoever ' you ' may be ? " If the latter is intended, and the pronoun is being used in the generalised way, the question becomes : " How can any one of us ever justify any inference to the feelings of someone other than himself, since no one of us, whoever he may be, has any means of checking any inference to the feelings of anyone other than himself ? " And to this generalised form of the question the commonsense answer again suggests itself : each and every one of us, whoever he may be, has the means of independently checking the reliability of the methods of inference which he uses, although, naturally, on those occasions when he needs to use any particular method of inference, he cannot be independently checking the inference on the same occasion. When I, Hampshire, check in my own experience the reliability of the various particular methods of inference which I use when talking about the feelings of others, the statements which I make at the conclusion of these checks are *ex-hypothesi* not themselves the conclusions of an inference ; but they are none the less efficient as checks to my methods of inference. The solipsistic problem, cleared of these confusions, can now be re-stated : whenever anyone uses the sentence " I feel giddy ", one person and one person only is in a position to know directly, and without need of inference, whether the statement conveyed is true ; whenever anyone says " you feel giddy ", or " he feels giddy ", or " Smith feels giddy ", one person and one person only is in a position to know without need of inference whether the statement is true ; whenever anyone says, " we both feel giddy ", or " they feel giddy ", no one can ever know directly, and without need of inference, whether the conjoint statements conveyed are true. So the solipsist may correctly say that it is a distinguishing characteristic of statements about feelings, as opposed to statements about physical things, that at most one person can ever properly claim to know directly, and without needing to give evidence or justification, whether such statements are true. But the solipsist originally wanted to separate, within the class of statement about minds, a class of statements about *other* minds, as being dubious

and problematical, from autobiographical statements, which were held to be privileged and not dubious. It is this distinction which is untenable.

Suppose that, in talking about our feelings, we each solipsistically confined ourselves to statements which we may properly claim to know to be true directly and without appeal to evidence or to methods of inference ; I, Hampshire, would be allowed to say " I feel giddy ", and you, Smith, would be allowed to say " I feel giddy " ; but, since all uses of other cases of the verb require problematical inference, we would never be allowed to assent to or dissent from each other's statements, or to place ourselves in the position of an audience discussing them. Under such conditions the pronouns and cases of the verb would have no further function, and all argument and the detection of lies would be excluded : our psychological language would simply serve to convey a set of undiscussable announcements. Communication in the ordinary sense upon such topics would have ceased ; for communication essentially involves the use of sentences to convey statements by an author to an actual or potential audience, in such a way that all users of the language, in denying and confirming, may change from the position of audience to author in respect of any statement made. To compare the use of personal pronouns with the uses of tenses again : because those statements which refer to events long prior or subsequent to the moment of utterance are *pro tanto* relatively uncertain at the time they are made, it might be suggested that only statements in the present tense should be accepted as completely reliable. But unless we recognise the sense of " same statement " as something to be re-affirmed in different contexts, we remove the last possibility of correcting and denying statements, and with this we remove the possibility of all argument about them and testing of them, and also the possibility of expressing belief or disbelief ; we therefore remove the essential conditions or point of statement-making ; and this we would have done by failing to recognise the function of those devices which relate the same statement to the changing circumstances of its assertion. The formula often used, " I am in a position to judge of the truth of statements about my own feeling, but not about the feeling of others ", has only succeeded in misleading, because of the two ways in which the expression " I " and " other " may be used, and the often unnoticed shift from one use to the other; it is this shift which suggests a solipsistic conclusion—*e.g.* that one mind only can be known with certainty to exist and one set of feelings and sensations known with certainty to have occurred. But

of course no such conclusion about *one* mind follows from the argument when correctly stated. The proper truism is, " No one of us, whoever he may be, is in need of inference to assure himself of the truth of statements about his own feelings, but he can never assure himself directly, and without needing to appeal to evidence, of the truth of statements about the feelings of others " ; stated in this form, with a quasi-variable expression as the subject term, the truism cannot serve as a premise to any *solipsistic* conclusion.

4. The peculiarity of the word " know " and of its cognates— that the conditions of their proper use in combination with any type of statement vary with the indicated context of utterance— is not confined to discourse about minds and feelings ; it applies over the whole range of application of words like " know ", " certain ", " verify ", with whatever kind of statement they are combined. Whatever may be the topic under discussion, whether a claim to knowledge or certainty is or is not in place, must always depend upon who makes the claim, when, and under what conditions ; it can never be solely a matter of the form of the statement itself or of its topic. Any empirical statement whatever is a matter of uncertain inference under some conditions of its use or consideration. There is no mystery in the fact that a statement which may be a matter of direct and certain knowledge for one person will always be a matter of uncertain inference for another, any more than there is mystery in the fact that the same statement which may be known with certainty to be true at one time must be a matter of uncertain inference at other times. Philosophers (Plato, Descartes, Russell) have invented the mystery by writing as if being known to be true and being uncertain were intrinsic properties of statements, properties somehow adhering to them independently of the particular circumstances in which they were made or considered. It is proper and necessary that formal logicians, who study patterns of transformations of sentence-forms, should disregard those features of statements which relate them to a context of utterance ; but philosophers' questions about use and meaning hinge on the different contexts in which words like " know " and " certain " may occur in combination with sentences of different forms and different topics.

5. *Conclusion.*—" Past ", " Present ", " Other ", are not class terms but contextual terms, and there can be no class of events which are past events, and no class of minds which are other minds, and no class of statements which are statements about either of these. " Statements about other minds " is either an

incomplete expression, requiring knowledge of the particular circumstance of its use in order that it should be intelligible— *e.g.* " minds other than mine, Hampshire's " ; or the contextual expression may be used in the generalised sense and mean " statements about minds other than the author's, whoever the author may be " ; if the latter is intended, then in raising the problem of other minds we are enquiring into the analogy which enables anyone to compare the situation in which he knows a statement about feelings to be true, independently of inference, with the situation in which he does not ; and it is to this comparison that we refer when we talk of checking the reliability of any method of factual inference.

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II.—ON DESCRIBING

By S. E. TOULMIN AND K. BAIER

DESCRIPTIONS can be as emotive as you please, and are never
' true or false '.

If this truism appears paradoxical, that shows only how far
the terms ' describe ', ' description ', ' descriptive ' and the like
have been distorted in recent philosophical discussions.

Unfortunately, philosophers have made these changes un-
wittingly, and have as a result been driven into defending
untenable positions. Indeed, the whole programme, in con-
nection with which these words have acquired their recent
philosophical currency, is ill-conceived. For the aim has been to
throw light on the status of controversial types of utterance,
such as moral ones, by drawing a single, sharp distinction ;
this has been marked by such pairs of words as ' descriptive ' and
' emotive ', ' descriptive ' and ' normative ', ' description ' and
' prescription ', and ' description ' and ' decision ' ; and has
frequently been identified with the distinction between the classes
of statement to which the epithets ' true ' and ' false ' can and
cannot be applied. Such a programme, it will be argued, rests on
a misunderstanding of the ways in which we classify our utter-
ances, and on an over-simplification of the process by which
language is made to serve our purposes.

I

In this first section, we shall consider briefly some of the
conditions which have to be fulfilled if we are to make proper
use of the verb ' describe '.

The reasons for concentrating on the verb ' describe ', rather
than the noun ' description ' or the adjective and adverb ' des-
criptive ' and ' descriptively ', will become clear as we go along.
It may be said at once, however, that these different parts of
speech can generally be used equivalently, so that the conditions
for the use of one will, *mutatis mutandis,* apply equally to the
others. Thus we can say, alternatively :

" In the first part of his paper, Rutherford describes his
apparatus " ; " The first part of Rutherford's paper describes
his apparatus " ; " The first part of Rutherford's paper is a

description of his apparatus "; or, less elegantly, " The first part of Rutherford's paper is descriptive of his apparatus "; and each of these ordinarily entails that the first part of Rutherford's paper is a description or is descriptive. Notice particularly that, when we say of a chapter, paragraph or passage that *it* describes so-and-so, we are imputing no activity to the passage itself ; for to say this is equivalent to saying that, in the chapter, paragraph or passage, *the author* describes so-and-so. This point is none the less important for seeming obvious and trivial. For people have sometimes tried to explain on what conditions a passage can properly be said to describe so-and-so by reference to the form of the passage, the sorts of words used in it, etc. *alone* : as we shall see, however, it is necessary to pay attention rather to the circumstances in which the author wrote or published the passage. And the same applies to the associated words ' description ' and ' descriptive '.

The sense of the word ' describe ' with which we are concerned is an unusually precise one. Indeed, the compilers of the *Shorter Oxford Dictionary* go out of their way to label it as ' the ordinary current sense '. They mention otherwise only such non-linguistic senses as that in which the figure of a gladiator can be said to be described upon marble, or a triangle upon a line, and that in which the sun describes a circle in the heavens. There are, nevertheless, two markedly different uses to which the word is put, which it will be necessary to keep distinct.

For our purposes, the more important use of the word is that in which John Doe can only be said to describe anything if what he writes or says consists of one or more complete sentences, mentioning *inter alia* a number of characteristics of the thing concerned. This use is also the one which most closely fits the *Shorter Oxford* definition : ' to set forth in words by reference to characteristics ; to give a detailed or graphic account of '. We may compare " John Doe described so-and-so to Richard Roe " with " John Doe reported on such-and-such to Richard Roe ", which also implies the use of at least one complete sentence; and we can oppose it to " John Doe welcomed Richard Roe " and " John Doe is teaching Richard Roe the alphabet ", which ordinarily imply the use of speech, but not necessarily of complete sentences.

On occasion, however, we use the word ' describe ', in the form of words ' describe . . . as . . . ', in a way which implies rather the use of a noun or noun-phrase. Thus, *Pravda* might be said to describe Marshal Tito as ' the servile lackey of the

Imperialist West ', a letter to the *Times* describe television as ' a menace to the mental health of the younger generation ', and a sophisticated tramp describe himself as ' a wayfarer '. This use is a very weak use, having little of the precision of the primary use. At its weakest, " John Doe *described* so-and-so *as* such-and-such " means only " John Doe *called* so-and-so ' such-and-such ' ", or " John Doe *said that* so-and-so *was* such-and-such ". It is accordingly important to keep these two uses distinct. We shall find later that, from the beginning of the recent philosophical discussion of ' descriptions ', these very different uses have been telescoped.[1]

The corresponding senses of the noun ' description ' can easily be distinguished. The primary sense is used to refer to passages or utterances consisting of one or more sentences, and calls for such epithets as ' detailed ' and ' graphic ': the weaker sense is used to refer to a noun or noun-phrase, and calls rather for such epithets as ' apt ', ' appropriate ' and ' grotesque '. To see just how different these senses are, notice that we might even have occasion to ask how far to use a particular description (noun-phrase) would be to describe its object, *i.e.* whether the description was at all descriptive ; and it might very well be held not to be at all so. For the question is : if the ' description ' (noun-phrase) is reformulated as a sentence or series of sentences and offered as a description (primary sense), how far will it be acceptable as one ? Suppose, for instance, that the noun-phrase ' the servile lackey of the Imperialist West ' is replaced by the sentence, " Marshal Tito's chief characteristic is servility, and his behaviour towards the Imperialist West is that of a lackey " : then we can always ask, how far this could be accepted as a description of Marshal Tito, in the primary sense of the word. And it is this primary sense on which we shall concentrate for the rest of this section.

Suppose, therefore, that John Doe has written or uttered some string of words : in what circumstances will it be appropriate to say that he was *describing* something ? Several different types of condition must commonly be fulfilled. These are concerned, respectively, with John Doe's audience, the topic or subject of his utterance, his position *vis-à-vis* his audience and subject, the purpose which the utterance is designed to serve, and the extent to which it does in fact serve this purpose. Let us consider these in turn.

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[1] *Punch* exploited this distinction some years ago (Jan.–June 1934, p. 374): *Policeman,* to man with black eye, " Can you describe your assailant ? " *Man,* "That's what 'e 'it me for, describing 'im ! "

Audience.—To say of John Doe that he is describing something implies that he has an audience. John Doe describes so-and-so *to Richard Roe.*

Consider what we say if this condition is not fulfilled. Suppose, for instance, that John Doe is alone on a mountain-top, and utters a string of sentences—either " Monte Generoso is 1700 metres in height and commands a magnificent view of the Alps : there is a funicular railway up the Swiss side, but easy tracks lead to the summit from both sides . . . ", or else " Here the elements struggle for mastery against a backcloth of rock and snow. How the knife-edge of the wind catches at our throats as we crest the lonely summit, while the dark caterpillar of the train crawls off round the shoulder. . . . " Are we to say in such a case that he is describing the mountain ? If not, how are we to classify his utterance ?

What we shall say depends upon a number of things. If he simply murmurs the words to himself, without any intention of presenting them to an audience—and especially if he makes no attempt to record them or commit them to memory—we shall say that he is soliloquizing, talking to himself about the mountain. If he says them over to himself slowly and several times, gradually improving or adding to them, intending later on to write them down and publish them, or to utter them to some friend down at the hotel, or to his club back home, we may speak of him as composing a possible description of the mountain. If he first jots them down and then declaims them to see how they sound, we may say that he is rehearsing his description of the mountain. One can think of many different cases.

Notice, however, that in none of the cases does one say that he *is* describing the mountain, for to do that immediately gives rise to the question, " To whom ? " Certainly in many of them one can speak of his words as constituting a (possible) description of the mountain, but to do so is to look forward to the time when the sentences he is composing or rehearsing or jotting down *will* be published, or uttered to an audience. In reporting the one case in which no question of later publication arises, one can hardly say more than that his soliloquy took the form of a description of the mountain—*i.e.* that in other circumstances it might have done duty as one. The words which he uttered to himself may be *the very words* in which he later describes the mountain to Richard Roe ; their *meaning* may be exactly the same in both cases ; nevertheless, when he first uttered them he was not describing the mountain. To describe a mountain to someone, is to do one thing ; to rehearse a description of it,

for later delivery to someone, is to do another; to soliloquize about it for your own private ear alone, in words taking the form of a description, is to do a third.

Topic.—If we are to use the word 'describe' to report what John Doe said to Richard Roe, it must make sense to ask *what* John Doe described, what was the topic or subject of his description. John Doe describes *so-and-so* to Richard Roe. The verb 'describe', in other words, must have an object; and a number of points arise about this object.

(i) Not anything and everything can be the topic of a description. We can describe persons and things, such as Mr. Gladstone, New College garden, a chair or a dress; events and incidents, such as the Opening of Parliament or a car accident; processes and techniques, such as Bessemer's method of steel production or how to make almond fingers; and much besides. But a fact, which is sometimes spoken of as though it were the topic *par excellence*, cannot be described: it can only be stated. Further, a statement of fact about something may sometimes constitute the beginning of description of that thing, as in "Their Christmas tree was decorated only with candles". But very often it will not: "Hurry up! Mother is waiting", or "It is ten past four". This is one indication of the differences between a description and a statement of fact. 'Description' is not a word parallel to the phrase 'statement of fact': it refers rather to a *type of use* to which a sentence may be put.

(ii) The thing described must be what it is held out as being. One cannot say in general what this means, but some illustrations will help to make the point clear. If the utterance purports to describe a certain material object, such as a house, chair, or dress, then this object must exist at the time the description refers to; if an event, happening, incident or episode, then this must have occurred at the material time; if a figure in a novel, a fairy-tale or a mythology, then there must really be such a figure in that novel, fairy-tale or mythology; if some process, method or technique, then there must really be such a process, method or technique. If John Doe describes Mr. Pickwick to Richard Roe on the understanding that he is telling him about a character in a novel, well and good; but if he lets it be thought that he is talking about his best friend, and so a real person, Roe may complain that Doe was only 'pretending to describe' someone. There is no King of France, no Greek god called Venizelos, no famous heroire called Francesca Pepper, no technique of intuiting; hence there can be no description of the King of France, etc. Someone who, thinking that there was, claimed to be

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describing one of them, would merely think that he was. In fact, he would either, and unwittingly, be describing something else which he had taken for the King of France, say, or else not be describing anything at all—however much what he said might take the form of, and purport to be, a description. Again, if John Doe empties his safe, ties himself to his chair, and rings for the police, the story he tells them about masked men stealing the firm's ready cash cannot be called a description of a robbery, for there has been no robbery. What he tells them is all made up.

(iii) At some point it must be specified what *sort* of thing is being described : characteristics alone do not ordinarily suffice. Unless either John Doe or Richard Roe has already indicated what is being, or is to be, described, John Doe's words must, *inter alia*, do so : " What I have lost is a six-week-old, tabby *kitten*, with a white waistcoat. . . . " If this is not done, John Doe's words will be understood, not as a description, but perhaps as a riddle : " It walks on four legs in the morning, two at mid-day, three in the evening. (What is it ?) "—though notice once again that a riddle, like a soliloquy or a false report, may take the form of a description.

Relative position of speaker and audience.—If John Doe is to be said to be describing something to Richard Roe, he must be in a better position than Roe to speak about it. One special case is important : we should not usually talk of Doe describing to Roe something which was there in front for both of them to see. We happily speak of a radio commentator as describing a football match to his unseeing audience, but are less happy to use the word of a television commentator, who follows the play for viewers, for the viewers can see what is going on. Yet the words used by the two men may be the same.

Thus, John Doe can describe the view from M. Generoso to Richard Roe, provided Roe has never been up the mountain, or never in such good conditions. But if they are together on the summit we could speak of Doe describing the view to Roe only if Roe were, say, blind, and so unable to see for himself. Again suppose that, after both seeing the view, they go home, and Doe writes a description of it, *e.g.* for delivery to his Rotary Club : if Doe reads this over to Roe, we can still not say that he is describing the view to him, for Roe knows the view quite as well as Doe. Rather we shall have to say some such thing as, that he is trying out his description on him.

Two apparent exceptions to this : (i) Even if they were both on the top of M. Generoso, with Monte Rosa clearly visible, one might yet in certain circumstances speak of John Doe as

describing M. Rosa to Richard Roe.   But this would be, perhaps, because Roe did not know which of the many mountains he could see was M. Rosa ; and John Doe would describe it for him so as to enable him to pick it out : " It's the one at the left-hand end of the chain, with a row of spiky peaks like a comb ". We can, that is, speak of Doe ' describing ' something to Roe, either if they both know what exactly is being described but Doe is or has been in a better position to observe it than Roe, or if both are equally well placed, but the auditor needs a description in order to identify that to which his attention is being drawn.

(ii) We learn at school about such things as ' the llama ' and ' Bessemer's method of making steel ', and may have to repeat to the teacher or examiner the descriptions from which we learnt about them.   Accordingly the examination paper may, for brevity, read, " 5. Describe the llama ".   But there remains something paradoxical about saying, " Johnny is describing the llama to his natural history master ", when the master knows so much more about the animal than he does.   Only if Johnny had come from South America or Whipsnade, or had worked in a steel-works, could he, in the full sense of the phrase, describe the llama or Bessemer's method to the teacher.

One rather complicated example—Suppose John Doe and Richard Roe have both seen the same incident, but separately and unknown to each other ; then Doe may talk about it to Roe afterwards, and say of himself that he is describing the incident to Roe.   Roe, however, may not be happy about calling Doe's words a description, feeling that Doe is in no position to describe to him an incident about which he knows quite as much, if not more.   He might prefer to say, for instance, that John Doe was giving him his account of the incident.   Here again, there need be no quarrel between them as to the exact *words* Doe used : the decision to apply or withhold the word ' description ' is made on other grounds.

Function.—Roughly speaking, we say that John Doe is describing something to Richard Roe only if the purpose of his utterance is of a kind which a picture might serve.   Many of the foregoing points arise out of this fact.

(i) The purpose of a description, and equally of a picture, is in many cases to act as an aid to recognition.   Thus a police notice, or a notice about a missing animal, will consist largely of a description of the wanted man or the lost dog.   And it will be a better description in proportion as it helps the reader to recognize the man or the dog.   Very often, indeed, a police notice will include a photograph of the wanted man.   And

further, the better the picture, the less need there is of a des-
cription. If, to take an extreme case, a coloured and moving
waxwork, uttering typical remarks in the appropriate accent, were
sent round to police stations instead of a 'Wanted' notice, it
need hardly be accompanied by any description at all.

In the case of descriptions of events and techniques, the same
sort of thing often holds. Thus the eye-witnesses of a car accident
may be called on by the police to describe the accident as they
saw it ; their descriptions will be the better, the more nearly
they will do duty for a suitably-taken ciné film of the accident ;
and if such a film of the accident were in fact available, the
descriptions would hardly be needed. Again, a speaker on the
wireless may describe a method of making Dundee cake ; the
description will be the better, the more nearly it will do instead of
a demonstration ; and, given a demonstration, one hardly needs a
description.

It is the purpose of such descriptions that explains why
their typical merits and demerits are what they are—namely :
exactness, minuteness, accuracy, detail, fullness, sketchiness,
misleadingness and so on. And the justice of the comparison of
descriptions with pictures will be seen if one notices how many of
the corresponding epithets, 'exact', 'minute', etc., could be
used equally of a drawing.

(ii) In other cases, a picture is drawn or painted in such a way
that likeness is wholly or partly sacrificed for the sake of some-
thing else. The picture is then a success if it is 'vivid', 'graphic',
'colourful', 'stirring', 'atmospheric', 'evocative', or 'moving'.
It serves its purpose, that is, not in the way that a passport
photograph, or the picture in a 'Wanted' notice does, but in some
other way.

The same is true of descriptions. The primary merit of a
description is not always that it is exact or detailed : at other
times, and for other purposes, we may value a description for
being colourful or vivid, for taking one in imagination to the
place described, for being a stirring description of a battle,
or for being moving, perhaps like Dickens' description of the
death of Little Nell.

In consequence, there is no more one and only one description
of anything than there is one and only one picture of it. For,
whatever we are talking about, we may want to do a number of
things involving it, any of which a picture or a description can
forward. The things which Baedeker chooses to record about
Florence in his description of it will accordingly differ from those
which a student of architecture and still more a poet would

include—just as a photograph of the Ponte Vecchio would differ from an architectural drawing of it, and that again from an Impressionist painting of it.

(iii) It is helpful to contrast those things which we do call descriptions (*i.e.* verbal substitutes for pictures, films and demonstrations) with other uses of language which, by themselves, we do not call descriptions. Thus to say of what sort a thing is is not necessarily to describe it ; to say where it is is not to describe it ; to tell someone the way from Oxford to Cheltenham is not to describe the road between those towns ; to say what happened is not necessarily to describe an event ; to say how you did something is not necessarily to describe your method of doing it.

(*a*) John Doe tells Richard Roe that he has lost his pet animal. Roe : "What sort of animal ? " Doe : "A zebra ". Roe finds a zebra straying half a mile down the road, secures it, and telephones Doe : " I've found what must be your zebra ". Here Doe has only told Roe of what sort his missing pet is ; he has not described it to him ; and correspondingly Roe cannot say, after encountering it, that he recognized it as Doe's zebra. To describe the zebra, Doe must mention salient and special characteristics for Roe to look out for, such as two pink stripes on the withers and a lop ear ; and only if he is told such things as these can Roe later say, " I recognized it as yours from your description ".

Where no classification is available (or known to the speaker), a description may be used *faute de mieux.* Doe : " I don't know of what breed my dog is, but let me describe it to you. It has the head of a spaniel, but the hindquarters of a sealyham. . . ." This use of ' description ' begins only where classification dries up. For to classify something, to say what it is, is to tell in what pigeonhole it belongs : to describe it, to say what it is like, is to say only what are the nearest pigeonholes, when there is no particular (known) one to specify. The commoner use of ' description ', by contrast, is to tell how a particular thing may be distinguished from, or recognized from among, others belonging in the same pigeonhole, or passing under the same common noun.

(*b*) John Doe invites Richard Roe to go and look at his pet zebra. Roe : " Where is it ? " Doe : " You'll find it in the paddock." Given this information, Roe will be able to locate Doe's zebra, and so, unless he finds several of them there, to identify it ; but once again he cannot claim to have recognized it, for Doe has only told him where it is, and has not described it.

(*c*) To tell someone the way from Oxford to Cheltenham is one thing, to describe to him the road from Oxford to Cheltenham is

another. An itinerary may be full of facts about the route,
yet we distinguish between this and a description of the route.
The latter will give one not so much instructions for getting
from the one place to the other, as a pre-view of the route. It
will tell one, for instance, that the road is winding and hilly,
but with a good surface, that it runs through wooded valleys to
begin with, changing later to bare uplands, that it passes this
striking church and that beauty spot, and so on.

(*d*) To tell someone what happened is one thing, to describe an
event to him is another. " A bomb hit the house " tells him
what happened. To describe the event one might say, " We
had been lying underneath the staircase all evening. Mother was
just saying to Father, ' I wonder whether we should go upstairs :
I think I heard the All-Clear ', when there was a terrific crash,
Father was thrown against the wall. . . ." Similarly, to say
how you did something may require only the words, " I used
Bessemer's method " or " I followed Mrs. Beeton's recipe " ;
whereas to describe your method of doing it would be a far
longer task.

The success of a description.—In order to describe something,
we must ordinarily mention a number of things about it. One
brush-stroke does not make a picture. Grammatically speaking,
therefore, a description is normally a complex affair. Accor-
dingly, we may ask whether the individual statements composing
a description are true or false, but we never ask of the description
as a whole, whether it is true or false. The questions which arise
in practice are always whether enough information has been given
for the purpose in hand, that is, whether the description is
sufficiently full ; whether it is accurate or accurate so far as it
goes ; whether it has been appropriately selected and presented,
that is, whether it is balanced, one-sided, or misleading, and so on.
The ways in which a description can be improved are, corre-
spondingly, by giving more information, correcting some of the
component statements, going into more detail on some points,
removing misunderstandings about others, and so on—and this is
quite different from (and a more complicated business than)
correcting a single statement. Where descriptions are concerned,
there is no place for the simple black-and-white distinction
between ' true ' and ' false ', so it is no wonder that in practice
we never apply these epithets to them.

What we frequently do instead is to use the words ' describe '
and ' description ' in a way which implies success. To speak
of John Doe as having described so-and-so to Richard Roe then
implies, not only that his utterance satisfied the conditions we

have already noticed, but also that it was sufficiently full, fair, accurate and well-balanced. If we feel that this is not the case, we can challenge his words by saying that he has misdescribed the thing—that what he said was a misdescription, never that it was a false description. This is a fairly general charge, and may preface a number of different sorts of criticism (the answers to the question " In what respect ? ") ; and it is also a fairly serious one, which we reserve for cases in which we can point to many or big inaccuracies, a misleadingly one-sided selection, etc. The charge that an utterance is a ' misdescription ' must, of course, not be confused with the statement that it is ' not a description at all ' : the one finds fault with what is said, the other with a suggested classification of the utterance.

There is, nevertheless, one use we do make of the phrase ' a true description '. When making a formal statement (*e.g.* to the police) we may have to sign a declaration to the effect that the facts stated constitute a ' full and true description ' of an event, say. But this is manifestly a declaratory use of the word : by signing, one certifies the description, and thereby commits oneself in various ways, laying oneself open to penalties if the information turns out to have been incorrect. There is no room in such a case for true to be opposed to false. To write out a description of something and sign under it the declaration, " The above is a false description of . . .", would be self-stultifying and lead to paradox in the way in which it would, if one were to use formulae like "I dishonestly assure you that . . ." or " I insincerely promise to . . ." One cannot give a description and challenge it, at the same time and in the same words. And notice: even if one challenges a description which someone else declares to be true, one will not have occasion to use the simple word ' false ', but rather such phrases as ' inadequate and misleading ', ' riddled with inconsistencies ', and ' a tissue of falsehoods '. Notice the metaphors : the description is a cloth made up of individual statements which are threads, or a shield, which is vulnerable, in so far as the individual statements composing it are false.

When checking individual items of information in the description, we may use the words ' correct ' and ' incorrect '. Thus, in John Doe's description of Monte Generoso, the mountain may be ' incorrectly stated to be, or described as ' 2,000 metres high. But it is the individual statements which are correct or incorrect, not the description as a whole : this we speak of rather as accurate or inaccurate. It is sometimes tempting to talk as though one could extend the use of ' correct ' and ' incorrect '

to the description itself, and accordingly to look for *the* one and only ' correct ' description. But there are no correct or incorrect descriptions except in the ' describe . . . as . . .' sense, the noun-phrase sense, in which to describe Princess Elizabeth as ' the heir-apparent to the throne of England ' is obviously to give a correct description of her. And notice that, when we use the phrase ' describe. . . as . . .' and the corresponding sense of ' description ', we do not imply anything about the truth, accuracy or correctness of what has been said : we do not, in this sense, oppose ' describing ' to ' misdescribing '. Whatever the height of Monte Generoso, John Doe can properly be said to ' describe it as 2,000 m. high '.

Our use of the phrase ' complete description ' must also be noticed. In the case of such things as inventories, verbatim reports and balance-sheets, there is a use for the pair of terms ' complete '/' incomplete ' quite distinct from that of the pair of terms ' finished '/' unfinished ', for we have a standard, an ideal, which such things must attain in order to be called ' complete ' : every item or word or asset and liability must be included. In the case of descriptions, this is not so : there is no end to the number of things we might put into a description of anything. So we ordinarily contrast a complete description with an uncompleted, interrupted, unfinished one. Sometimes, it is true, we may be asked, " Have you completed your description ? " or " Is this a complete description ? " meaning not " Have you finished saying what you intend to say ? " but " Have you included all the relevant information you know ? " or " Have you mentioned everything required by the authorities ? " But it always makes sense to talk of adding to a description, making it fuller than it is : the ideal of a complete description as one which enumerates *all* the qualities of the thing described is accordingly an illusion.

II

To give an exhaustive account of the notion of a description would be a very large task : it is enough for our purposes to show in outline what sorts of job the word ' describe ' and its derivatives have. And one thing comes out even from a cursory examination : namely, that the words ' describe ', ' description ', and ' descriptive ' mark off, not a particular class of sentences or words, still less a particular kind of subject-matter, or a type of effect that an utterance may produce, but rather a particular kind of way in which we *use* sentences and words.

The counterparts of the word ' describe ', that is to say, are such words as ' declare ', ' tell ', ' record ', ' report ', ' account for ', ' explain ', ' predict ', ' interrogate ', ' reprimand ', ' soliloquize ', ' harangue ', ' inform ' and ' pray '. Each of these words marks off a particular sort of linguistic activity or performance, in the course of which sentences and words of *all* kinds may be used, so that the same sentence may well appear at one time in a description, and at another time in a report or a soliloquy. And, just as the same sentence is not confined to a single role on all occasions, so also it may have more than one role on a particular occasion of use : a report on the state of a mine may contain a description of the mine, and one can sometimes reprimand someone simply by describing to him how his behaviour appeared to others. Different kinds of linguistic performance are, therefore, not necessarily exclusive one of another. At the same time not all of them are compatible with one another : a soliloquy, as we have seen, can never be a description, however much it may take the form of one.

Among the sorts of classification in which the words ' describe ', ' description ' and ' descriptive ' do *not* belong are the following : *grammatical* classification, as indicative, imperative, adjectival, interrogative ; *stylistic* classification, as poetic, slang, technical, flowery ; classification by *subject*, as geological, anatomical, scientific, legal, sporting, diplomatic, moral, economic ; and classification by *effect*, as boring, ineffective, discouraging, moving, stirring, heart-rending, blood-curdling. Nor do we say anything about the *meaning* of a passage by calling it a description.

There seems no reason to suppose that the categories in any two of these systems of classification need coincide : accordingly, for all that logic can show, it is possible to perform any linguistic activity, using passages containing each grammatical type of word and sentence, in any style, on any subject, with any effect. Certainly some combinations are incongruous, for practical reasons : one would rarely have occasion to soliloquize in geological terms, and it would be an ineffective harangue which consisted entirely of rhetorical questions expressed in sporting slang. Still, any of these things might be done, and several common and familiar possibilities are of philosophical importance.

A description, for instance, need not consist entirely of sentences in the indicative. One learns at school to interlard one's indicatives with imperatives, interrogatives and so on, so as to vary the style. This can produce a tiresome effect if badly managed, but few descriptions of any merit stick resolutely to

the indicative throughout.   In any case, one does not say that a
passage or utterance which contains a sentence in a mood other
than the indicative is *ipso facto* any less of a description, any less
descriptive : the things that lead us to speak of it as a description
are things about the circumstances in which it was uttered or
published, not things about its grammar.

Again, a description may appear in a work on any subject, and
be expressed in any kind of terms : there is no list of words which
are allowed to appear in descriptions, or of words which are barred
from them.   The reverse is also true : thus a paper on physics
will usually consist, partly of a description of an experimental
apparatus, partly of a report on the results of an experiment
made using the apparatus, and partly of a theoretical discussion
of the significance of these results.   One can have geological,
anatomical, legal and sporting passages and utterances which are
descriptions, and others which are not.   One can have a geo-
logical description of the Galapagos Islands, a geological report
on their mineral resources, or a geological explanation of the
peculiar rock-formations there.   Moral terms, likewise, are not,
as one might suppose from what some philosophers have written,
confined to harangues, reprimands, instructions, condemnations
and the like : indeed, descriptions of character demand to be
expressed in moral terms—" He is a saintly, courageous, kindly,
and conscientious old man, to whom duty is a pleasure ", or
" He is an unprincipled rascal, with an eye for the main chance
alone ".   And conversely, there are plenty of commands,
reprimands and so on which are expressed in other than moral
terms, and turn on other than moral considerations : instructions
for the use of a coal-cutter, an adverse report on a brand of face-
powder, and the like.

A description need be none the less a description for being
emotive.   Not all descriptions are bald and factual, any more
than all pictures are photographic ; and some, like Dickens'
description of Little Nell's death-bed are designedly moving.   The
effect which an utterance has on the reader or hearer is one thing,
the kind of linguistic performance it represents is another, and
a description no less than a story can be either boring or blood-
curdling.

Two passages meaning exactly the same may one of them
constitute a description, the other not ; and the same words may,
without alteration of meaning, constitute a description on one
occasion of use or to one hearer, but on another occasion or to
another hearer not.   This was brought out earlier, when we
noticed that the same words might constitute first a soliloquy, and

later a description, or first the rehearsal of a description, and later the description itself. Notice that, when we say that a particular soliloquy took the form of a description, we imply, not that it was a description, but rather that in other circumstances the same words could have served as a description.

'Descriptive' is an adjective neither of grammar, of style, of effect, nor of meaning: in the same way, a classification by subject is distinct from one by effect or style or meaning. A geological report may be depressing or heartening, and a moral defence may be vehement and winning or dispassionate and convincing. And there is something paradoxical about Stevenson's suggestion that one can identify the meaning of an utterance with its effect, even on suitably-conditioned hearers.

If we consider the wide use, 'describe . . . as . . .', and the corresponding 'noun-phrase' sense of the word 'description', the situation remains materially the same. If John Doe describes something as such-and-such, his description can be expressed in any terms, in any style, produce any effect and have any meaning. It may be appropriate or inappropriate, correct or incorrect, flattering, neutral or insulting, and designed to anger or appease or to do neither. We can describe something either as an unsuccessful attempt or as a praiseworthy effort, and someone either as a 68-year-old recidivist or as a villainous old rascal. Descriptions, in the noun-phrase sense of the word, are as often commendatory and condemnatory as descriptions in the primary sense.

To sum up: there are several different ways in which words and sentences, passages and utterances, can be classified. These cut across one another. One may classify a passage on grammatical and stylistic grounds, by subject, effect and meaning, and still leave open the question what kind of linguistic performance it constitutes; whether it was a description or not. The converse is also true. One can even interrogate someone without using interrogatives: "I put it to you that you then struck him with the hammer—deny that if you can!" And we all know those heart-rending descriptions full of sentences in the Dickensian imperative: "Struggle on, game heart, to sustain the last embers of life in the little frame!" And all this is independent of the further question, what sorts of passage and utterance it does, and what sorts it does not make sense to criticise as false, or to defend as true.

III

It has already been seen that the term ' description ' applies ordinarily, not to sentences as such, but to one of the uses to which sentences can be put.   Mr. Strawson [1] and Mr. Warnock [2] have already drawn attention to this distinction in the cases of the terms ' refer ' and ' point ', and have shown how serious can be the consequences of ignoring it.   And further, it should be clear by now that the distinction between describing and other types of linguistic performance and activity is a fairly subtle one.

In recent analytical philosophy, however, it has often been taken for granted that the terms ' describe ', ' description ' and ' descriptive ' are so simple as hardly to need examination, that they can be reserved for some class of words and sentences, as such, as opposed to a class of uses to which we can put any words and sentences in appropriate situations, and that they can be used at the same time to mark distinctions of grammar, of logic, of subject-matter, of psychological effect and of meaning.

In order to see how this has come about, we must go back a little into recent history, and see how the technical use of the term ' description ' has developed.

Some features of this technical use of the term spring originally from the work of Kirchhoff, Mach and Pearson [3] who applied it to scientific statements, laws and theories.   Their point in so doing was to contrast scientific explanations with metaphysical ones, and to rebut the view that the task of science is to find hidden causes, and to reveal ' necessities in nature ' :   Pearson in particular stresses the contrast between scientific laws, which are ' descriptive ', and civil laws which are ' prescriptive '.   It was certainly not their aim to identify the terms ' scientific ' and ' descriptive ', or to suggest that they could be used interchangeably, as I. A. Richards later came to do.   Nor could they consistently have done so, for they all agreed in regarding predictions as scientifically respectable pronouncements ;   and predictions are not necessarily, and are perhaps never, descriptions. Nevertheless, one does find in their work the first, harmless deviations from our familiar idea of a description.   Sometimes they treat the term as equivalent to ' simile ' or ' model '.

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[1] " On Referring ", MIND, July, 1950.

[2] " Empirical Propositions and Hypothetical Statements ", MIND, January, 1951.

[3] Cf. E. Mach, *Popular Scientific Lectures*, pp. 190-225, and K. Pearson, *The Grammar of Science*, esp. ch. 3.

Sometimes they use it more as we do the term ' record ' ; and this last use leads naturally on to Carnap's use of the term ' *Protokol* ' for the fundamental type of descriptive statement.

Another influential use of the term was that made by Russell in his famous Theory of Descriptions. Russell called phrases such as ' the author of *Waverley* ', ' the highest mountain in Nebraska ', and ' the present King of France ' by the name of ' definite descriptions '. He had first called them denoting phrases, but abandoned this name because one of his main points was that such phrases may denote nothing ; and the phrase ' definite description ' seems to have struck him as more appropriate. He probably did not consider, or care very much, whether his use of this phrase was in line with our ordinary use or not. Nevertheless, he certainly tried to give the impression that it was. Consider the example he makes use of in his *Introduction to Mathematical Philosophy*, p. 167—" ' Who did you meet ? ' ' I met a man.' ' That is a very indefinite description.' " This is clearly intended to be understood in the ordinary, non-technical way. But the question asked, " Who did you meet ? " is not a request for a description. Nor is the reply, which properly answers the question, a description, but a very general and uninformative classification of the person met. One might, of course, if asked to describe the person you met, say in reply, " All I can tell you is that, to judge by the clothes, it was a man " —but this would amount to an admission that you could not comply with the request. Again, Russell seems to be following ordinary usage when he says (p. 140), " There are innumerable correct descriptions of any given object ". But at this point he shifts from the stricter sense of ' description ', which applies to a passage consisting of at least one complete sentence, to the very wide ' noun-phrase ' sense of the word, derived from the form ' describe . . . as . . .' : " Socrates ", he writes, " can be *described as* ' the master of Plato ', or as ' the philosopher who drank the hemlock ', or as ' the husband of Xantippe '." (Our italics.) And thereafter he uses ' description ' for *any phrase* that could ever be used to classify, describe, identify, refer to . . . something or someone, rather than for the use of such a phrase to describe it, or him. As a result, the conditions we examined in section I of this paper are all ignored : ' describe ' comes to be used, in the way ' describe . . . as . . .' is more commonly used, as equivalent to ' make a statement, whether true or false, about . . .' This foreshadows the technical use of ' description ', now common, to cover any sentence which can be spoken of as true or false.

Quite as important as Russell's use of the term 'description' is the model of ' describing ' introduced by Moore about the same time.   Consider the following quotation from his essay, " The Conception of Intrinsic Value " (written before 1917, and published in *Philosophical Studies*, 1922):

> " I can only vaguely express the kind of difference I feel there to be [between ' intrinsic properties ' and ' value predicates '] by saying that intrinsic *properties* seem to describe the intrinsic nature of what *possesses* them in a sense in which predicates of value never do. if you could *enumerate* all the intrinsic properties a given thing possessed, you would have given a *complete description* of it, and would not need to mention any predicates of value it possessed ; whereas no description of a given thing could be complete which omitted any intrinsic property."   (Our italics.)

Notice two things :

(*a*) Moore says (or implies) that, if asked to describe something, one would not use value predicates alone in one's reply. This is true.   One would not say, for instance, " It is good, beautiful and priceless ".   And if one did, one could not be said to have given much of a description of the thing in question.

(*b*) It is a different matter, however, to suggest that value predicates have no place at all in descriptions of things.   One would not use value predicates alone, certainly : but one would not use size predicates or odour predicates alone either—predicates of several kinds would generally be needed.

Notice, therefore, that he also says (*The Philosophy of G. E. Moore*, p. 590), that " in ascribing to a thing a property which is not a natural intrinsic property [*e.g.* intrinsically good], you are not describing it *at all*, whereas, if you ascribe to a thing a natural intrinsic property, you always are describing it to some extent, though, of course, the description may be very vague and very far from complete."   Here he implies that to describe something is simply to ascribe to it certain kinds of property, *i.e.* to apply certain classes of adjective to it.   And this explains the italicized words in his earlier paper ; for Moore's model of describing can now be recognized.   A description is an ' inventory of the intrinsic properties possessed by the thing described ', and a ' complete ' description one which ' enumerates all these properties '.   This model of Moore's drops many of our normal requirements about descriptions, but introduces a fresh one, for in this inventory only certain kinds of property are to be listed : namely, those which are ' intrinsically possessed by ' the thing. From this it is a short step to saying that certain kinds of adjective are out of place in a description.

This next step was taken, a year after the publication of Moore's *Philosophical Studies,* in Ogden and Richards' *The Meaning of Meaning* (1923) : they conclude that certain words, *i.e.* those which 'stand for natural intrinsic properties ', should be taken as peculiarly descriptive, and others, such as moral and aesthetic words, as peculiarly non-descriptive.   Thus on pp. 124-25 they say :

> " Amongst these [very subtle dangers] is the occurrence, in hitherto quite unsuspected numbers, of words which have been erroneously regarded without question as symbolic in function. The word 'good' may be taken as an example . . .   The word stands for nothing whatever [*i.e.* corresponds to no item in Moore's 'inventory '], and has no symbolic function. . . .   It serves only as an emotive sign expressing our attitude . . ., and perhaps evoking similar attitudes in other persons, or inciting them to actions of one kind or another."

The authors do not say whether certain words are descriptive entirely and others emotive entirely, or whether all words are both descriptive and emotive, but in different proportions. They do, however, say quite plainly that, in certain uses, certain words are purely emotive, *e.g.* 'good' and 'beautiful '.

With this step, the technical use of the terms ' descriptive ', ' description ' etc. assumes its present form.   At its most extreme, this use rests on the following doctrine.   There are two large classes into which sentences, and the words which figure in them, can be divided.   On the one hand, there are those sentences, to be dignified by the title of ' statements ', which express propositions ; which are the concern of the sciences, and of those everyday activities which are like the sciences in having to do with facts and the stating of facts ; which express beliefs ; which are properly couched in the indicative mood ; and whose meaning consists in the cognitive or rational effect which their utterance has on a suitably-conditioned hearer's beliefs.   These, and these alone, can be spoken of as true or false, according as the propositions which they express do or do not correspond with the facts.   On the other hand, there are those sentences, to which it is advisable to deny the title of statements, which do not express propositions ; which are the concern of, for instance, ethics, aesthetics, poetry and cognate activities ; which express or evince attitudes ; which are only misleadingly couched in the indicative mood ; and whose meaning consists in the affective or persuasive effect which their utterance has on a hearer's attitudes. Since these sentences do not express propositions, there is no question of their being true or false.   It is to the sentences in the first of these two large divisions and to the words which can appear

in them, that the words 'descriptive' and 'description' have
recently come to be applied. They have been spoken of and
written about as 'descriptive statements and words', and con-
trasted with the 'emotive (normative, persuasive, prescriptive,
imperatival, performatory) expressions' of the second group.
Alternatively, they have been labelled as 'descriptions', and
contrasted with the 'decisions', 'exclamations', 'ejaculations',
'prescriptions', 'proposals', 'formulae', etc. of the second
group.

IV

The divergence between the ordinary uses of the words
'describe', 'description', 'descriptive', etc. examined in
section I, and their technical use, whose development was sketched
in the last section, is so great that it does not need to be underlined.
But three things are worth emphasizing. First, the divergence
seems as a matter of history to have developed gradually, so that
the writers responsible for each fresh departure probably made it
unwittingly. Secondly, from the beginning of this development
the wide use, 'describe . . . as . . .', and the precise use have
been telescoped. Thirdly, the very paradigms of a 'description',
in the technical sense—" this is red " and " this is a table ",
said when the thing concerned is currently under observation—
would, in the ordinary sense, qualify for the title only in ex-
ceptional circumstances and then only by courtesy, since the
hearer would have to be blind, or otherwise prevented from seeing
the thing, and the words would have to be but the beginning
of a longer utterance. And perhaps it is worth repeating, yet
again, that the technical use makes the use of the term dependent
on the sort of words used ; whereas in the ordinary sense any
words can in appropriate circumstances figure in a description.

" No doubt ", someone may reply, " this is all very true, and
even interesting. Perhaps the terms are indeed used as you have
argued. And perhaps there are divergences between the ordinary
and the philosophical uses. But that does not mean that the
philosophical use is a farthing the worse. The most it shows is
that philosophers have tumbled on rather an unhappy word to
mark the distinction they are interested in. So all that you
can ask is that they replace the terms 'descriptive' and 'des-
cription' by others : 'informative' or 'fact-stating', and
'assertion' or 'statement' would do as well instead."

To this suggestion there is a two-fold reply. To begin with,
let any words be chosen to take the places of 'descriptive'
and 'description', the situation remains as bad as ever ; for the

distinction which they are required to mark is itself illegitimate. And in the second place, to drop the words ' descriptive ' and ' description ' for others would actually only make matters worse ; for all that serves to hide the illegitimacy of the distinction is the veil provided by the ordinary associations of these words.

These points must be explained. The philosophical distinction between descriptive statements and other sentences is illegitimate in an interesting way. For no one could say that there was no distinction here to be drawn. On the contrary, there are nine or ten, *viz.*

    (i) the distinction of subject ; between the natural sciences, the social sciences, ethics, poetry, aesthetics, etc. ;

    (ii) that of grammar ; between sentences couched in the indicative mood and other moods ;

    (iii) that between sentences with different meanings :

    (iv) that between describing and other types of linguistic performance and activity ;

    (v) that of manner, style or mode of expression ; between the dry, flowery, technical, emotive, etc. ;

    (vi) that between arguments relying for a great or small part of their weight on logical and careful reasoning ; between the reasoned and the merely persuasive ;

    (vii) that of effect, between the boring, blood-curdling, etc., or, in the jargon of psychology, between the cognitive and the affective ;

    (viii) that between beliefs, feelings, attitudes, etc. ;

    (ix) that between the sorts of sentence and passage of which one can use the words ' true ' and ' false ', and others ;

    (x) that between contexts in which one can use such phrases as ' a matter of fact ', ' that's a fact ', etc., and those in which one cannot.

These distinctions themselves probably need sub-dividing ; and we have not listed the further, technical distinctions between sentences which ' express propositions '/' correspond to facts '/ can be called ' statements ', and other sentences—for they serve only to enshrine the doctrine to which we object.

The trouble is, that these distinctions cut across one another. They are distinctions of different kinds, are drawn on different grounds, and in consequence cannot be expected to cut along the same line. There may be a few occasions on which we utter a sentence or argument which falls in every respect on one side or the other of the Great Divide. But this is certainly not always so, as the examples produced in section II indicate. When faced

3

with a controversial class of utterances, which falls across the
Divide, such as moral, aesthetic, mathematical, judicial, or
ritual utterances, philosophers have too often reacted by con-
solidating some or all of these distinctions, and arguing as
though they cut along the same line. But the result of
welding these nine or ten different distinctions, or a selection of
them, into a single, monolithic distinction is not to clear away the
philosophical fog surrounding such utterances : it only thickens it.

Further, the arguments used to justify the division of all
utterances into descriptive and others commonly involve appeals
to the ordinary senses of the words. And only in this way are
the paradoxical conclusions drawn from it given an appearance
of inevitability. Thus Ayer, in the course of a paper on " The
Analysis of Moral Judgments ",[1] writes : " A valuation is not a
description of something very peculiar ; it is not a description
at all." And this reads very plausibly, for of course, as we
ordinarily use the words, a valuation is indeed one thing, a
description another. Valuing (as a professional valuer does it)
is indeed a different activity from describing, although a typical
valuation does contain descriptions of the objects valued. Even
if we understand ' valuation ', not in its everyday sense, but as
meaning ' calling something " beautiful ", " good ", " right ",
etc. ', the appeal is still plausible ; for the conditions on which
something can count as a valuation, in this sense, are more
stringent than those on which it can count as a description,
in the ordinary sense. It must now be expressed in a special
vocabulary, that is, in moral or aesthetic terms.

It is only when the word ' description ' is consciously taken in its
technical sense that the claim, that ' a valuation is not a des-
cription at all ', becomes clearly questionable. For in the
technical sense of the term, to agree to this commits us to some
fair proportion of the following views : that moral terms are
necessarily out of place in descriptions, that moral utterances are
all normative, that one should not use the terms ' belief ', ' fact ',
' true ' or ' false ' in connection with moral utterances, that they
are not really statements, and that they are only misleadingly
couched in the indicative. But these are the very paradoxes
which the appeal to our common-sense views about descriptions
were intended to justify. The argument is, in other words, of the
familiar form : ' Such-and-such does not constitute a des-
cription, in the ordinary sense ; only descriptions, in the technical
sense, have the feature so-and-so (e.g. can be spoken of as true or
false) ; ergo such-and-such has not got the feature so-and-so '.

[1] Horizon, Sept. 1949, p. 179.

This can be confirmed if we consider the suggestion that the words ' description ' and ' descriptive ', in their technical sense, should be replaced by some other words. For if we try to use, in their place, any of the words which it is natural to suggest as alternatives—' informative ', ' fact-stating ', ' assertion ' or ' statement '—appeals to the ordinary uses of these words no longer carry the same weight ; they are ordinarily far from equivalent to ' description ' and ' descriptive '. Whereas it is plausible enough to say, " A valuation is not a description of something very peculiar ; it is not a description at all ", it is far less obvious that a valuation " does not give one information at all, not even information of a very peculiar kind ", that it " tells one no facts ", or that it " is not an assertion ". So the fact that philosophers have used the terms ' description ' and ' descriptive ' to mark their Great Divide is not just an unhappy accident : it is, rather, the one thing which gives the Divide some appearance of respectability.

Nor is the resemblance between Ayer's view that " A valuation is not a description at all " and Moore's view that, in calling something ' intrinsically good ', " you are not describing it *at all* " an accident either. For both conclusions are the result of thinking about adjectives as the names of properties, not just in a manner of speaking, but so strictly that you regard them as ascribing to objects properties which reside in them, and which could, in principle, be enumerated in an inventory or complete description. The pity is that, when Ogden and Richards pointed out the error of regarding ' good ', ' beautiful ' and the like as ' symbolic in function ', they did not recognize that it was quite as much of an error to regard ' red ', ' big ' and the like as ' symbolic in function '. That would have spared us many tears.

V

Many analytical philosophers to-day would refuse to subscribe to some part at least of the Great Divide. Nevertheless, it has left its mark, even on those who would reject the more paradoxical conclusions drawn with its aid. For one can see, easily enough, the impossibility of drawing, at a single stroke, all the ten or more distinctions which are concertina'ed in the extreme form of the doctrine ; but it is not so easy to disentangle completely the many things which it entangles. One pair of distinctions, in particular, remains obstinately glued together ; that between describing and other linguistic activities and performances, and that between utterances of which we can and

cannot use the words 'true' and 'false'. (This is probably
the effect of the secondary form, 'describe . . . as . . .',
acting *via* Russell's 'theory of descriptions'.)

Thus Mr. Hart, in writing about judicial decisions, says : [1]

> " Since the judge is literally deciding that on the facts before him a
> contract does or does not exist, and to do this is neither to describe
> the facts nor to make inductive or deductive inferences from the
> statement of facts, what he does may be either a *right* or a *wrong*
> decision or a *good* or *bad* judgment and can be either *affirmed* or
> *reversed* and (where he has no jurisdiction to decide the question)
> may be *quashed* or *discharged*.   What cannot be said of it is that it is
> either *true* or *false*, logically necessary or absurd."

And again he writes,[2] of the statement " Smith hit her ", that

> " If, on investigating the facts, it appears that we should have said
> ' Smith hit her accidentally ', our first judgment has to be qualified.
> But it is important to notice that it is not withdrawn as a false state-
> ment of fact. . . .   Our ascription of responsibility is no longer
> justified in the light of the new circumstances of which we have notice.
> So we must judge again : *not describe again*."

In both these passages, it is assumed that only when we
describe the facts can the things we say be true or false, and
that to deliver a verdict or ascribe responsibility is incompatible
with deciding the facts.   In each case this assumption leads to
paradox.   For if Smith accidentally strikes Mrs. Jones, and
Jones later comes and says to him " You hit her ", Smith will
certainly feel entitled to reply, " that's not true : it was an
accident ", and to insist that Jones has been misinformed as to
the facts.   This is not to deny that, as Mr. Hart illuminatingly
remarks, to say " Smith hit her " is to ascribe responsibility to
him, in a way in which to say " Smith hit her accidentally " is
not.   In the case of many judicial decisions, the paradox is still
more marked.   For, whatever may hold in contract cases, if in a
murder trial the foreman delivers the verdict, " We find the
prisoner *Guilty* ", the prisoner may protest, " It's not true !
I didn't do it ! ", with perfect logical propriety.   One may, of
course, consider a verdict ' simply *qua* verdict ', *i.e.* ask whether
it was properly arrived at, and so restrict oneself to the question
whether it was good or bad, whether it should be affirmed or
reversed, accepted or appealed against.   But it is in the nature of
the law that verdicts should often imply things about what
happened at the material time : so considered, they can very
naturally be criticised as false (or should it be ' not true ' ?).

[1] " The Ascription of Responsibility and Rights ", *Proc. Arist. Soc.*,
1948-49, p. 182 (reprinted in Flew, *Essays on Logic and Language*, p. 155).
    [2] *Op. cit.*, p. 193 (Flew, p. 165).

Only so long as one supposes that different types of linguistic performance are necessarily *exclusive* of one another can one conclude that a verdict, being, as it were, a performatory utterance, is thereby disqualified from being true or false.

It is true, as Mr. Hart says, that the Judge is not required " to make deductive or inductive inferences from the statement of facts ". But he is none-the-less required to make the *appropriate* kind of inferences : and, as with all inferences, we can ask two kinds of question about the conclusion, " Is it true ? " and " Does it follow ? ". Only by confining our attention to the latter question can we keep the word ' true ' out of the picture. But of course it *makes sense*, however improbable it may be, to say of the best and most carefully-drawn judgments that it was a miscarriage of justice.

Mr. Austin, too, wishes to restrict ' true ' and ' false ' to descriptive statements, and opposes these to performatory utterances, value-judgments and the rest. Thus he writes : [1]

> " Recently it has come to be realized that many utterances which have been taken to be statements (merely because they are not, on grounds of grammatical form, to be classed as commands, questions, etc.) are not in fact descriptive, nor susceptible of being true or false. When is a statement not a statement ? When it is a formula in a calculus : when it is a performatory utterance : when it is a value-judgment : when it is a definition : when it is part of a work of fiction—there are many such suggested answers . . . It is a matter for decision how far we should continue to call such masqueraders ' statements ' at all, and how widely we should be prepared to extend the uses of ' true ' and ' false ' in ' different senses '. My own feeling is that it is better, when once a masquerader has been unmasked, *not* to call it a statement and *not* to say it is true or false."

And, to make no mistake, he has gone out of his way to call ' performatory ' utterances, like the words " I do " as used in the marriage service, by the name of ' non-descriptions '.[2]

Here again the last traces of the Great Divide can be seen. To begin with, grammatical form is accepted as a *prima facie* ground for regarding an utterance as being descriptive ; as being susceptible of being true or false ; and as being genuinely a statement. When these four independent distinctions are found not to cut cleanly along the same line, the offending utterances are accused of masquerading and thrust into outer darkness. But it is not the countless different ways in which we ordinarily classify utterances that are open to criticism : what is misconceived is, rather, the attempt to run them together.

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[1] *Arist Soc. Supp. Vol.* xxiv, 1950, p. 125.
[2] The title of a paper read to the Cambridge University Moral Sciences Club, 1947.

As well might a man confound the distinction between cabbages and lettuces with the distinction between cabbage lettuces and cos lettuces ; insist on calling lettuces 'non-cabbages'; and dismiss the term 'cabbage lettuces' as a misnomer.

Forms of words and vegetables alike, poor things, are incapable of masquerading. But we who classify them, for our own purposes, may be confused about our own classifications. Just how our different distinctions are drawn, is something that no one has ever clearly stated. The first thing is to recognize this fact : then we can begin on the laborious but indispensable task of coming to understand our own linguistic techniques.

III.—PROBABILITY-SENTENCES

By Ernest H. Hutten

1. A probability-sentence is taken to be a declarative sentence in which the term " probability " or one of its derivatives occurs. The definition of this concept entails that the occurrence or non-occurrence of an event referred to in the probability-sentence is compatible with any degree of probability, and so it seems that such sentences can never be verified (or falsified) since a probability equal to unity is not certainty and zero probability is not impossibility. This usage marks off the distinction between the concept of probability and that of truth.

The view that probability-sentences are factual but not empirically decidable requires scrutiny. To accept into science sentences that are in principle incapable of empirical verification is clearly not desirable, and our objection need not be due to philosophic prejudice. For inductive method requires the use of probability-sentences, and induction is a guide to action. If probability-sentences are supposed to be factual but not verifiable by any fact, this would imply a strange interpretation of induction, and its value for science would certainly be diminished.

2. Probability-sentences are formulated in a great variety of ways which are all compatible with the rules of standard grammar. For the purpose of analysis such sentences must be reconstructed in logical terms so that we may obtain a clearer grasp of their content. In other words, we must try to specify the rules according to which probability-sentences may be formulated and used, so that differences between various sorts of such sentences can be noted. This logical reconstruction need not exactly coincide with actual formulation but it must approach it to a reasonable degree. Since the usage of probability-sentences in ordinary language is vague (like many other expressions the probability-sentences are often used there to convey emotive rather than logical meaning, *e.g.* credibility) it is best to restrict the discussion to scientific, or near-scientific, contexts. Only after the problems arising in this well-defined context have been cleared up can we expect the analysis of probability-sentences in the ill-defined context of ordinary language to be profitable.

It appears that a rough classification of probability-sentences is as follows. There are two standard forms in normal usage.

First, there is the *inductive* sentence which *predicts* the future occurrence of a single or of many events, *e.g.* (A) " It is probable that it will rain to-morrow ". Inductive sentences may also *postdict* a past event, *e.g.* (A') " It is probable that Caesar crossed the Rubicon ". The second form of probability-sentence is the *frequency* sentence which *describes* the actual occurrence of a property within a (finite or infinite) collective of similar events, *e.g.* (B) " The probability of obtaining one point with one throw of a single (true) die is 1/6 ". This latter formulation, however usual, is already somewhat misleading. For the probability is assigned to a specific, given, die ; and when we speak of *a* single die we implicitly introduce the inductive hypothesis that any single die behaves in this way. So (B) must be interpreted with caution if we wish to separate the two kinds of sentences.

There is, of course, a third form in which the adverb " probably " occurs, *e.g.* (*A''*) : " It will probably rain to-morrow ". More often than not this kind of sentence reduces to the sentence (A). The modal character shows that the sentence refers not to actual but to possible future events, and so it is of predictive import. It is, however, not necessary to deny that on occasion this sort of sentence may also be construed as being of kind (B).

The difference between the two kinds of probability-sentences is shown in the now widely accepted distinction between the two concepts of probability. Inductive probability (or degree of confirmation, or logical probability, or probability of hypothesis) is explicated in terms of the overlapping ranges of two sentences within a semantic language-system.[1] It is a function whose arguments take sentences as values. Frequency probability (either as ratio within a finite class or as limit of a ratio within an infinite sequence) is a function whose variables are classes of physical events. The two functions are of different logical type and so they are entirely different functions. They have a mathematical calculus in common to some extent and they are possibly based upon the same set of (uninterpreted) axioms but differ in interpretation.

This difference is seen also in other ways. Inductive sentences often do not specify numerical values. Frequency sentences always do so, or at least the qualifiers " more " or " less " occur. In inductive sentences the unqualified term " probable " often suffices, and so such sentences are taken to express a somewhat weakened assertion, or warranted assertibility (to use Dewey's phrase).

[1] Carnap, *Phil. of Sci.*, vol. xii, 1945. Hempel, MIND, vol. liv, 1945.

The inductive sentence normally uses a predicate—*i.e.* the two-term predicate " probable "—while the frequency sentence employs the noun " probability ". Although this is not a strict distinction it is quite noticeable. There seems to be a preference for the noun when events are described, while the adjective or adverb is preferred in predictions.

Finally, the two kinds of sentences differ in tense. For the inductive sentence (or, rather, the hypothesis contained as partial expression in it) is always about the future or the past ; and the sentence as a whole assigns a probability to the hypothesis on the basis of evidence. The *pure* frequency sentence describes actual occurrences, and so states a probability as a physical property of events on the basis of comparison with other events.

The main difficulty arises when the frequency sentence is used to make a prediction of future happenings. The sentence (B) is often construed as (C) : " The probability that face 1 *will* show when a single die is thrown is 1/6 ". Although the future tense is not explicit in the sentence (B) it is implied in the phrase " of obtaining " which is normally taken to refer to the future, *i.e.* it has not been obtained yet.

Another difficulty arises from the verbal habit of formulating probability-sentences as *incomplete* sentences and so their usage is elliptical. This has brought about the so-called *absolute* concept of probability, or the concept of *chance* which seems to play a role in every-day life. But the analysis of the concept of probability has shown that such sentences are logically meaningful only if they make use of a *relative* concept. Inductive probability is assigned to a sentence (*i.e.* a hypothesis) relative to another sentence (*i.e.* the evidence). Frequency probability is attributed to a class of events (*i.e.* as a property) relative to another class (*i.e.* the reference class). To be meaningful a probability-sentence must be complete, and so probability has to be taken as a relation of at least two terms. When not explicitly mentioned the second term of the relation is tacitly implied and this makes for ambiguity, for the sentence in question may be completed in at least two different ways.

It seems useful, then, to divide probability-sentences into inductive (predictive) and descriptive sentences, although in fact most probability-sentences function as both in actual usage. Such a division is at least a first step towards elucidating rules of usage for probability-sentences, even if it may turn out that in practice the division is not unambiguous. There can be no objection to this attempt, and there is no serious difficulty in separating the two characteristics. " Description " must always

refer to an *actual* state of affairs while " prediction " is concerned with the future. This trivial distinction must be kept in mind, for otherwise it will be impossible to separate the nearly ubiquitous characteristics of induction (and prediction) from that of description. The results of previous induction and past experience are pervasively present, the more so since we are usually concerned with action, and thus with inductive behaviour. Whenever we say that such and such *is* the case, we are ready to imply that the same will hold for the future, and so our psychological propensities introduce an unexpressed Principle of Uniformity and, with it, induction.

3. The classification of probability-sentences can now be given in more detail. The probability-sentence may be of (*a*) the inductive, or (*b*) the frequency kind ; it may involve (*c*) prediction, or (*d*) description ; and it may refer to (*e*) a single event, or (*f*) an indefinitely large number of events.

A probability-sentence of the pure frequency kind always refers to a large number of events, since this follows from the definition of " frequency probability ". The relative frequency of any property can, of course, be observed only in a finite segment of a sequence. Bernoulli's theorem shows that, under certain conditions, the value of the relative frequency will remain within prescribed limits when the sequence is continued indefinitely. It is argued sometimes that this confers a predictive character to a probability-sentence of this kind.

Such a conclusion is incorrect. When a mathematical theorem such as Bernoulli's is *applied* this must involve an additional hypothesis about facts. That any *actual* sequence of events can be so described, or that the mathematical formalism is applicable, is naturally an assumption. When the frequency sentence is taken as holding for the unobserved continuation of the sequence it is no longer a *pure* frequency sentence. Rather it incorporates the prediction in the form of a hypothesis that the observed frequency will remain stable in the actual, future, sequence. In other words, the concept of frequency probability is not applicable for prediction without presupposing induction. This, of course, has often been noted.

An inductive conception of relative frequency probability has been formulated by Reichenbach in order to overcome this objection.[1] But is such a conception permissible ? It entails that the existence of a limit must be formulated as a Principle of Induction, and to be able to apply inductive method at all

[1] Reichenbach, *The Theory of Probability*, University of California Press, 1949.

becomes a necessary condition of predictability. For the definition of "predictability" " . . . will turn out to entail the postulate of the existence of certain series having a limit of the frequency ".[1] This is an ontological assumption, for it is tantamount to prescribing a property nature must possess. It makes no sense to say that only those events are predictable whose sequence has a certain property, for it is just this property (as well as other properties) which we might wish to predict. Prediction is possible, but this is not because nature, or certain sequences of events, possess specified characteristics. And induction is not a search for suitable causal sequences but a logical method of confirming sentences about them.

The future continuation of an *actual* sequence can never be postulated to be Bernoullian, *i.e.* to follow a mathematical law, even if the frequency is stable in the observed segment of this sequence. Such behaviour cannot be prescribed, only conjectured. A hypothesis is introduced that can and must be tested by observation, and confirmed on the evidence so obtained. The concept of relative frequency probability alone does not suffice to ascribe a predictive character to a probability-sentence, and its inductive conception achieves prediction only at the cost of prescription.

Of course this Bernoullian argument falls in with the prejudice of traditional epistemology according to which inductive inference leads from limited evidence to a more comprehensive hypothesis by the Principle of Induction. Bernoulli's theorem, when used as such principle, is a synthetic sentence, since it then says something about future events. It means that induction becomes an illegitimate deductive inference disguised as a mathematical theorem. Whenever probability is defined in terms of relative frequency in the long run, or as a limit, or in any way as referring to an as yet unknown future, it must introduce a hypothesis about this future whenever it is applied to actual instances. When used only within a mathematical calculus the limit is merely a mathematical expression formed according to certain rules. A limit can never be more than a convenient notation, and there are no objections to its use. On the contrary, the invention of infinitesimal calculus has been the main support of modern science. But it is a matter of interpretation when the calculus is applied. Bernoulli's theorem, since it speaks about the continuation of a sequence according to a rule (otherwise there would be no limit) is interpreted to hold for the future of actual sequences. But a deductive relation can never be

[1] Reichenbach, *Experience and Prediction*, Chicago, 1938.

predictive. If the relative frequency concept is used for pre-
diction and induction it requires an additional hypothesis in
order to guarantee the stability of the frequency, and this changes
the character of the probability-sentence. A pure frequency
sentence is only descriptive.

Of course, we are rarely interested in purely descriptive
probability-sentences in scientific practice. When we gather
statistical material we mean to use it for prediction. However,
weather records or mortality statistics for a given year say
nothing about what is going to happen the following year, unless
a suitable hypothesis is introduced. And this hypothesis
need not express the stability of observed frequency if the
mortality statistics concern a year in which a serious epidemic
occurred. Statistics by themselves are merely a convenient
manner of presenting data.

That most probability-sentences of the frequency kind are also
used for prediction and so contain another concept of probability
seems to be one reason for well-known difficulties of the definition
of probability as " relative frequency in the long run ", or of the
limit conception. The sequence described in terms of this prob-
ability must satisfy two contradictory conditions : it must
possess a limit, *i.e.* there is a rule of formation for the members of
the sequence ; and it must be random, *i.e.* there is a general
independence between the members. The limit character
obviously belongs to a mathematical sequence, and so is part
of the symbolism used. Randomness belongs to the physical
sequence of events so that it may be taken as statistical ; other-
wise the sequence is causal in the strict sense, or deterministic :
for it is the absence of a deterministic law which allows us to
describe the sequence by a probability law. The failure to
separate the two concepts of probability confuses the rule of
formation of an expression with the condition of its applicability.

It follows from the definition of " relative frequency
probability " that the concept cannot be applied to the *single*
case, and all are agreed in this matter. In order to manage
with one concept of probability Reichenbach has tried to apply
frequency probability to the single case by assuming a *fictitious*
meaning to the probability-sentence so used. A *posit* is made,
i.e. a wager, by which a *weight* is assigned to the single event,
and the weight is said to be the probability for the single event.

But this is again equivalent to introducing inductive
probability. A hypothesis is made about the single event and its
probability is *estimated* on the basis of statistical evidence (the
observed frequency). This sort of interpretation does not keep

to the pure frequency concept, and indeed the use of the term "posit" shows that we speak about a physical property, or estimate its value. An estimated value is not the actual value of the property. A frequency sentence referring to a single event is, strictly speaking, meaningless, or incompatible with the definition. Only by introducing another interpretation, or another concept—*i.e.* the concept of inductive probability (or logical probability)—can the sentence be made meaningful. But it is necessary to separate the two concepts since they are of different logical type, and are used according to different rules.

The inductive conception of relative frequency probability confuses two different concepts, or two ways in which the probability-sentence is used. That for such interpretation the existence of a limit follows tautologically does not mean that actual sequences do behave in this way. The problem of induction is not solved by prescribing the course of nature.

4. If the probability-sentence is of the inductive kind, what is then the correct analysis ? The sentence (A) " It is probable that it will rain to-morrow " expresses the probability of a future event, or it assigns a probability to a hypothesis. A prediction is made in the form of the hypothesis " It will rain to-morrow " and its probability is judged on the basis of evidence ; neither the degree of probability nor the evidence are explicitly mentioned. This is a standard form of a probability-sentence of the inductive kind, and such sentences always refer to some unknown state of affairs.

This probability-sentence can be regarded as the *conclusion* of an inductive inference which is not explicitly made. The suppression of the evidence, *i.e.* in this instance the weather records and similar knowledge, tends to obscure the logical character of the sentence. According to the modern conception induction is a non-demonstrative inference relating two sentences, and the rules are given by the calculus of probability suitably interpreted, so that it applies to the semantic language-system in which the sentences are formulated. The conclusion of this inference is thus a sentence stating the degree of probability assigned to the hypothesis on the evidence. The sentence of confirmation is derived by inductive logic and is analogous to the sentence in deductive logic asserting an implication. Traditional epistemology assumes a universal synthetic sentence as Principle of Induction. In the rationalist schema of induction this is used as a major premiss to simulate a syllogism. In the empiricist version it is used as rule of enumeration to bridge

the gap from known evidence to the unknown, more com-
prehensive, hypothesis. And in both instances the rule so
introduces ontological assumptions and prescribes the course
of nature. The epistemological view of induction must thus
be abandoned in favour of the logical reconstruction of inductive
method. The terms "confirmation" and "probability im-
plication" have been used in recent literature to distinguish
inductive from deductive (syllogistic) and epistemological
inference.

Whether the hypothesis refers to a single event or to an
indefinite number is immaterial, and in fact the hypothesis as
well as the evidence is often about statistical phenomena.
Whenever the probability-sentence expresses a *judgment* it must
be construed as being of the inductive kind.

The sentence judging the probability of a hypothesis on the
basis of evidence is obtained by the rules of inductive logic, and
so the inductive probability-sentence must be *analytic*. This
sentence is analytic with respect to the language-system in
which it is used since the probability relation between the
two sentences is determined by the rules of a calculus. We
need not even know whether the hypothesis and the evidence
are actually true or false. If we know their logical meaning
(*i.e.* their *possible* truth), and so their logical ranges, the degree
to which these overlap can be computed.

The difference between varying degrees of universality ascribed
to a hypothesis (or law) illustrates the use of the concept of
inductive probability. Only *instance* confirmation is of import
for a genuine prediction. Laws construed as unrestrictedly
universal hypotheses (*i.e.* holding for all times and for all events)
are used as analytic sentences : they define the terms occurring
in them. But we can speak of the probability of a law only
if it is used to predict the *next* occurrence of an event. In science
laws (in the simplest instance, universal hypotheses containing
a single universal quantifier) function in both these ways, and
the example of the Conservation laws is well known. This
difference, however, shows, once more, that probability-sentences
containing a prediction cannot be interpreted in terms of relative
frequency. Frequency probability cannot say anything about
the next occurrence of an event, or about a single case.

When we predict an event with a certain degree of confirmation
and the event fails to occur, what do we say ? The empirical
character of the hypothesis and of the evidence seems to confer
some sort of factual import on the inductive probability-sentence.
But this failure may embark us on two different courses of action.

We may abandon the hypothesis if the evidence is considered reliable or we may try to improve the degree of confirmation by searching for more evidence. What we do is not a matter of logical rules but of experience. In other words, the actual truth of the hypothesis (and of the evidence) does not affect the degree of confirmation, and so the inductive probability-sentence can only be analytic (in the language-system used). The sentence " It will probably rain to-morrow " is not affected by the fact that it does or does not, rain to-morrow. Only success in practice can ultimately justify a given inductive method.

5. If the sentence stating the degree of probability of a hypothesis is analytic, how can we use it in practice ? This raises the problem of semantic interpretation for the probability-sentence.

Probability is, first of all, a mathematical concept. Its reconstruction is given by an axiom system and *syntactic* rules for the usage of the symbols which provides a formal *calculus*. Kolmogoroff's axiom system in terms of set theory (a logical or mathematical interpretation of the axioms) is generally regarded as the best version, although there are others. The syntactic rules attached to the system are chosen in view of a possible semantic interpretation. That is to say, the forms of expressions to be admitted to the calculus will depend to some extent on the meaning we wish later to attribute to them.

The required interpretation of the calculus, in terms of experience, is given by *semantic* rules. A semantic language-system, or *theory*, of probability is constructed so that the mathematical expressions can refer to the physical world (or, in general, to something outside the calculus). Rules are sentences (*i.e.* are verbal), they are not formulæ of the calculus, and so they mention the expressions and the physical events by using their names. In this way the object-language of the calculus is interpreted by a known meta-language taken to refer directly to experience. The meaning of the expressions of the calculus is thus established, and this involves the semantic conception of truth. For to know the meaning of a sentence is to give a rule of truth for it (*i.e.* its truth conditions relative to the language system), and in this manner the calculus is interpreted.

Frequency probability is then a *physical* property, or two-term relation, attributed to an event sequence with respect to another such sequence. The sentence " $P(A, B) = p$ " means that the property A occurs within a sequence B with the relative frequency p. Inductive probability is a *logical* property, or two-term relation, attributed to one *sentence* with respect to another sentence within a language-system. The sentence

" $dc(h, e) = p$ " means that the hypothesis h is confirmed by the evidence e to the degree p.

Both the hypothesis and the evidence are synthetic sentences (in the language-system chosen), for they are used to refer to extra-linguistic facts. The hypothesis contained in the sentence (A) refers to the occurrence of rain to-morrow and the suppressed evidence refers to weather records and similar information. The sentence (A) can be reconstructed as : " The hypothesis of rain to-morrow is confirmed by the meteorological evidence to the degree p ". The inductive probability-sentence is then also concerned with physical events, although indirectly. The frequency sentence speaks about a physical property of a sequence of physical events ; the inductive sentence speaks about a hypothesis about physical events and judges this hypothesis : it is an estimate of the occurrence of the property (or of an event) relative to known instances of occurrence. Frequency probability is a physical property of events, inductive probability is a semantic property of sentences.

Inductive probability has been said to be a semantic version of frequency probability, and this is supposed to mean that the probability of a hypothesis is obtained by a translation from the probability of events.[1]

This argument meets with logical difficulties. To replace the physical event by its name is to construct a different series of events, for the members of this series are the marks (or letter-designs), and this is not the sequence we want. We must describe each event, and this is not merely a matter of naming nor do syntactic rules suffice for this purpose (*i.e.* it is not a translation). It is an interpretation involving the use of semantic rules for the event must be replaced by the *assertion* that it has occurred. The name-frequency is not a truth-frequency, and the individual words do not represent the " truths " of the sentences since truth is not a thing and cannot be counted. Only by abandoning the semantic conception of truth can the two concepts of probability be made to merge.

Grammatical usage has contributed to this confusion. When a hypothesis is judged by evidence, we should say - " It is probable that the probability of obtaining one point with one throw of a single die is 1/6 ". The phrase " it is probable that the probability " and similar iterations, are shunned by grammar and so are contracted in speech. This distinction, however, is vital for a satisfactory analysis of probability-sentences and of induction.

[1] Reichenbach, *Joun. Unified Sci.* vol. viii, 1938. Von Wright, *The Logical Problem of Induction*, Helsinki, 1941.

The distinction tends to be obliterated also for the reason that many hypotheses are statistical, as in the example from dicing. It is general practice to take as estimate of a relative frequency (*i.e.* as probability of a hypothesis) a number close to, or the same as, the expected value of the observed frequency. In recent writings on the theory of confirmation this usage has been reconstructed as the *best estimate*, and so its scale or metric, is determined by a definition. To return to the example : the estimated value of the probability of obtaining one point with one throw of a single die is made to approach the actual frequency of this occurrence in the long run. For we know of course that, with a true die and under normal conditions, the relative frequency is stable so that by continuing the series of throws we increase the evidence favourable for the hypothesis. And this is the reason (dictated by evidence) why the scale of the estimate is chosen according to the convention given.

This need not, and cannot, always be so but must depend on the kind of hypothesis and evidence we deal with. Not all statistical sequences possess a constant frequency probability but their probability may change with continuation of the sequence. Such variable frequency sequences are quite common : *e.g.* we subdivide the main event sequence into sub-sequences (say of equal length) so that each of them has a constant frequency for a certain character, and so a sequence of varying frequencies is constructed. How we obtain the best estimate of such a frequency sequence will depend upon their distribution : we might take the mean, or the most probable value, etc.

The scale, or metric, is always a matter of definition and so there is no question of an *a priori* judgment. Nor can the objection be upheld that a Principle of Uniformity is thereby tacitly introduced into the theory of confirmation and of induction. To specify a metric is a mathematical matter ; and the uniformity or stability of a frequency (either suggested by the evidence or as an arbitrary assumption) is justified only by success in application.

This is illustrated by the well-known statistical method of *maximum likelihood.*[1] The method requires three different kinds of information : the statistical evidence, a mathematical law of distribution, and a hypothetical population of which the evidence is a sample. The problem normally consists of finding that hypothetical population for which the known evidence is the most likely sample.

[1] Levy and Roth, *Elements of Probability*, Oxford, 1936. Fisher, *The Design of Experiments*, London, 1949.

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This method has sometimes been mistakenly said to be inductive but it is purely mathematical. The hypothetical (or original) population is not inductively inferred from the sample but constructed by means of a law of distribution (Gauss, Bernoulli, Poisson, etc.) according to which the sample is assumed to have been selected from the hypothetical population. Of course, once the hypothetical (or original) population has been constructed it represents evidence useful for induction because we can add the hypothesis that the next sample obtained in the same sort of experiment will also belong to the population in question.

The method of sampling is convenient for presenting data, and it furnishes a metric. For the evidence (*i.e.* a known distribution) is used to adjust the parameters of the hypothetical population so that this known sample is the most likely one. We are thus provided with a general method for introducing a metric into statistical material. The best estimate as discussed previously then appears to be a special instance of the application of this method. The relative frequency in the long run is the most likely value, or best estimate, if the distribution law according to which the hypothetical population is constructed is simply a *constant*. If the distribution law is Gaussian, so is the hypothetical population, etc. In other words, the constant frequency of the event sequence—the datum—is the most likely sample of a hypothetical population of frequencies which all have the same value. This then is the so-called Uniformity of Nature in the metric of inductive logic : it is merely a mathematical definition chosen, according to rules, on the basis of evidence. The choice of syntactic rules is thus governed by the possibility of interpretation, or by semantic rules, since for a true interpretation the two kinds of rules must coincide (*cf.* construction of semantic systems).

If the hypothesis refers to a single event no such convention is readily available. What scale is to be applied to the ranges of two sentences (within a semantic language-system) is, however, as before, a matter of convenience and convention. The practical difficulties at least at the present time, are everwhelming since descriptive semantics has not been sufficiently developed to allow the formulation of sentences (for the hypothesis and the evidence) as needed in application. The language-systems so far exemplified do not suffice in logical richness, but the simple systems which have been worked out illustrate that it is in principle possible to define a metric for them. The choice will obviously be guided by the decision to make the probability of a hypothesis

(or degree of confirmation) approach our judgments in practice, and the choice of the metric as well as that of the language-system as a whole can only be justified by success.

That it is in practice impossible to specify a suitable metric for inductive probability-sentence concerning single events is, I suppose, one reason why inductive logic, or at least the possibility of a numerical value for inductive probability, has been so often rejected. Since a sufficiently rich descriptive semantic system cannot be constructed as yet, simpler models must be used, approximations, etc., and so we are often forced to fall back on some sort of psychological intuition for judging hypotheses. A better logical reconstruction of inductive method may be hoped for with the development of descriptive semantics.

Still another difficulty arises from the multiple interpretation of " probability " in terms of relative frequency. The sentence (B) " The probability of obtaining one point with one throw of a single die is 1/6 " refers to a property of an event-sequence. There is a related sentence (D) about a property of a frequency-sequence : " The probability of obtaining a probability of 1/6 in repeated trials—consisting of series of throws with a single die, say, and resulting in a 1—is very nearly unity ". The probability of obtaining one point in a long series of die throws is 1/6 ; but the probability of obtaining sequences of die throws having this property is close to 1.

It is not always clear, in some formulation of probability-sentences, whether a sequence of events or a sequence of frequencies is concerned ; whether the reference is to a class or to a class of classes ; whether we speak about a sequence or a sequence of sequences. A sequence of sequences may itself be a sequence selected from the main event-sequence according to a rule, and so this probability is again a relative frequency. This has sometimes been called " a probability of higher order or kind " and the term refers to the frequency in finite segments of the main event-sequence. In the discussion of statistical problems text-books, most disconcertingly, switch from one kind of sequence to the other since both the event-distribution and the frequency-distribution are needed to solve such problems. In this instance the word " trial " refers to a large but finite number of events, and the trials represent a sub-division of the infinite (long-run) event-sequence. The frequency-distribution is then linked to the event-distribution.

When the limit-conception of relative frequency probability is used, we may have so to speak an infinity of trials of infinite length, i.e. an infinite frequency-sequence. Consequently each

frequency is obtained from an infinitely extended event-sequence, and the sequences may be independent of one another. An example is easily constructed. Reichenbach has used the term " higher level probabilities " to distinguish them from the finite case.

Such probabilities are sometimes said to be of special import in inductive inference. Reichenbach's theory of probability-lattices and of the hierarchy of posits makes induction depend upon the convergence of series of higher level. But regardless whether or not such probabilities refer to finite or infinite sequences, their role in induction is not different from that of ordinary frequency probabilities. The phrase " the probability of the probability . . . " is not synonymous with " it is probable that the probability . . . " so far as prediction is concerned. A class of classes, etc. is not equivalent to a sentence about a class, or a sentential function (propositional function) is not the same as the asserted sentence (statement, proposition). The sentence (D) says that the frequency of occurrence of an event-frequency of value 1/6 is, under given conditions, nearly 1. But this cannot be taken as confirmation of the hypothesis that certain experiments with a single die will result in a probability of 1/6 (the die may be loaded). There is still involved an asserted sentence such as " This event (or frequency) has occurred ", but this brings in the semantic conception of truth.

In many problems we deal with event-sequences of constant probability and, moreover, its value is 1/2 since we usually like to speak of mutually exclusive alternatives judged (from previous experience) or assumed to be equi-possible. The yes-or-no question is the one we like to ask, and the use of two-valued logic contributes to this attitude. So the co-ordinated frequency-sequence has a probability close to unity, and thus suggests a Principle of Uniformity. But this principle in its universal form is devoid of empirical meaning and can, at best, refer only to the uniformity of the *description* of nature. In application it is used to refer to the specific uniformity of selected character-istics, or events, in contrast to others. And then the principle is an empirical hypothesis requiring confirmation each time it is used. The thing-language of ordinary life is a constant-property language and widely applicable since some constancy of some property can nearly always be found with good approximation. All these factors conspire in obscuring the logical status of probability-sentences.

To summarize the argument. The analytic character of certain probability-sentences (here called " inductive ") has

often been noted. The conclusion of an inductive inference cannot be proved to be true nor even probable, and so must be analytic. Such a view does not create difficulties, but rather rescues us from the traditional puzzle. If inductive probability-sentences were empirical, there would be an infinite regress in inductive method (which is acknowledged in the frequency interpretation). This might not be so serious. But the empirical character of such probability-sentences also entails that the rules of inductive inference are used as *synthetic* sentences, and this is impossible. For it implies assumptions about the world or prescribes the course of nature. Unless the sentences stating the conclusion of an inductive inference are analytic (for a given language-system) no logical reconstruction of inductive method seems possible.

6. It is now clear that the verifiability of probability-sentences is not simply to be denied. We must first determine in which context they occur, or according to which rules they are to be used.

If the probability-sentence is purely *descriptive*, *i.e.* states an observed frequency then it is, of course, an empirical sentence, and so it is verifiable, not indeed by observing a single event but by observing a large number of similar events. This follows from the definition of "frequency probability". (If an infinite sequence and the limit conception is used, the limit is merely a convenient notation to represent the constant probability, and there is no need to observe an infinite number of events.) The given series of die throws has a certain, rather *elusive since collective*, physical property. This property does not belong to the single die alone nor to the single throw, but to the whole sequence of throws with a particular die. This is a description of a fact and cannot be construed as prediction. A loaded die does not possess the same frequency and this is easily decided by appeal to experience. It may, of course, require a large number of observations. This is the difference as compared to other empirical sentences for which a single observation suffices to test the presence or absence of a given property.

A probability-sentence of the pure frequency kind is descriptive, factual, and completely verifiable by experience, in the same sense in which any factual sentence is verifiable. If finite sequences are concerned, the frequency of any property can, in principle, be found by counting. If infinite sequences are used, the limit of the relative frequency represents merely a convenient notation and has by itself no predictive import : it must be read as the value of the frequency within a certain interval, for the finite

number of observations that can be made, and so it is again completely verifiable if the interval is included. Only if the limit of the frequency is used to refer to an *infinite but actual* sequence, then it is not verifiable, but in this instance it includes an inductive hypothesis, and this changes the character of the probability-sentence.

A probability-sentence of the inductive kind, *i.e.* a sentence judging a hypothesis on given evidence, is analytic, and so no verification by experience is required or indeed possible. The inductive probability-sentence, in its full formulation, contains two sub-sentences (the hypothesis and the evidence) both of which are used as synthetic sentences. But this does not mean that the probability-sentence as a whole must therefore be synthetic : it is simply not *used* to say something about the world. This task is already performed by the hypothesis and also by the evidence—one sentence saying something new about the world and the other saying what is known. And the inductive probability-sentence is used to judge whether this new knowledge is warranted by the known data. Such a judgment says nothing new about the world but is the result of knowledge, *i.e.* the hypothesis, in which it is already implicit, and inductive rules make it explicit.

A probability-sentence of this kind is the conclusion of an (often implicit) inductive inference, and it is agreed that this conclusion cannot be proved factually true, nor even probable. There are no epistemological reasons for this as is sometimes argued and no ontological assumptions are involved, but merely logical reasons. The failure to give an adequate analysis of inductive inference is mainly responsible for the thesis of non-verifiability. This shows up also in the quite common interpretation of sentence (A) where the whole inductive probability-sentence is taken to represent the hypothesis.[1] " It is probable that it will rain to-morrow " is read as " Rain is probable to-morrow " or " Rain is probable " (without exact reference taken for granted in the context, as is the evidence) ; and this sounds like " Rain is wet ". There is the suggestion that probable rain is a species of rain. Just as epistemologists often believe that existence is a property, so they take probability (or possibility and modality in general) as a property. And this mistake is the more understandable since " probability " does in fact denote a collective property in the sense of " relative frequency ", and the two senses are confused.

The two main epistemological theories, *e.g.* rationalism and

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[1] Kneale, *Probability and Induction*, Oxford, 1949.

empiricism, have incorrectly analysed inductive method by assuming a synthetic Principle of Induction, either as major premiss (in a syllogism) or as a rule of inference. And so the conclusion appeared to be a synthetic sentence, namely the hypothesis, supposed to have been derived by means of the Principle. The hypothesis is regarded as a " generalization of the evidence " or as an " abstraction from the facts ", obtained by the touch of a magic wand (the Principle) which transforms the meagre data into some new, and more comprehensive, knowledge about the world. But this epistemological view of induction is untenable and must be replaced by the scientific conception in terms of semantics. The hypothesis does indeed represent new knowledge, provided it is confirmed by the evidence; but no Principle can help us to discover it. The hypothesis is an invention of the scientist psychologically suggested by experience, and inductive rules check it against the evidence. So the conclusion of an inductive inference is not the hypothesis but the sentence judging it on the evidence, *i.e.* the inductive probability-sentence ; and both the hypothesis and the evidence are synthetic sentences available before the judgment can be made. If the hypothesis is confirmed on the evidence, then it does represent new knowledge, and this confirmation is expressed by an analytic probability-sentence. Only in this indirect sense can induction be said to provide knowledge. To find a hypothesis (or the evidence) is a matter of experience, skill, or luck—and not of logic. Thus the inductive probability-sentence states a logical relation between two sentences, or a semantic relation since the meanings of the two sentences are involved. Inductive method applies a probability-calculus to sentences according to tautological rules of inference, and the correspond- ing probability-sentence is analytic.

Consequently, it cannot be maintained that probability- sentences are factual but incapable of factual verification. Some probability-sentences are analytic (*i.e.* the inductive kind) and others are synthetic (*i.e.* the frequency kind). This has given rise to the view that all probability-sentences are peculiar, in the sense that they are not verifiable. And to make this view more agreeable it is said that to ask for verifiability is anyway a metaphysical demand.

It is most unprofitable to engage in an argument about meta- physics since this term is too ambiguous and its most common use is as invective. In books purporting to be about metaphysics we find, however, typical sentences ; and they are of this kind, that they are supposed to be factual yet incapable of factual

verification. Probability-sentences have been thought to be
metaphysical in this sense.

The thesis of verifiability is not metaphysical. Though
often given as part of some more modern school of philosophy,
such as empiricism or logical positivism, it is best not formulated
in terms of traditional epistemology. No ontological assumptions
need be involved. There are at least two parts to the thesis.
First, empirical sentences must be, in principle, capable of
empirical verification. Second, the meaning of an empirical
sentence is given by the method of its verification (this is a
traditional formulation). A better version is : To know the
meaning of a sentence is to know the conditions under which
it is said to be true. In other words, the meaning of a sentence is
given by the syntactic and semantic rules according to which it
is used within a semantic language-system. In this semantic
interpretation of the verifiability thesis no assumptions are made
about what we take experience to be. On the contrary, it is
merely a logical reconstruction of what we take as " the meaning
of a sentence " in ordinary language.

That all sentences are meaningful according to their usage, *i.e.*
the rules, is not a matter of philosophic prejudice. That factual
sentences must be, in principle, capable of factual verification
is no more than a definition of the term " factual " (" empirical ",
" synthetic ") in its normal usage. Those probability-sentences
which are empirical are verifiable, and their meaning is given by
semantic rules, and analytic probability-sentences possess
meaning given by *syntactic* rules. To say that probability-
sentences are empirical but incapable of verification is to ascribe
meaning to them without giving a rule : but how could they
be used then ? The proposers of such a view have to show that
sentences can have meaning and yet no rule of usage can, in
principle, be given. For this reason it is sometimes said that
inductive sentences constitute a third class apart from synthetic
and analytic sentences. But the classification of declarative
sentences into synthetic and analytic is again according to their
rules of usage. Although there may be difficulties in practice, that
is in a specific instance, all probability-sentences can be so divided,
and so they are meaningful. Their actual use is then re-
constructed by descriptive semantics.

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IV.—LOGICAL FORM

By Donald Kalish

Introduction

THE subject of this paper was suggested by Russell's assertion that forms are the subject matter of philosophical logic.[1] Russell and many other philosophers who employ the term " logical form " talk as if logical form is a property of propositions, and propositions are non-linguistic entities which are the meanings of sentences. Their point of view is suggested immediately by considering logical analysis—the activity of philosophical logic— as directed toward discovering these forms. By putting the matter this way they suggest that prior to an analysis there are things which in fact have forms, and that it is the logician's business to discern the forms that these things have.

In this paper, in contrast to the above-mentioned point of view, it is maintained that logical form is a property of sentences. To support this view the process by means of which forms are dis- covered is considered, and this consideration suggests a definition of " logical form " such that logical form is a property of sen- tences. Some examples, which I believe have intrinsic interest of their own, are employed to illustrate the thesis that logical form is a property of sentences, and to make clear that there is no question of *the* correct logical form of a sentence but that there are means for deciding between suggested alternatives.

1. *Problem Stated*

That philosophical logic is concerned with form is the tra- ditional and still prevalent view. Also traditional and still prevalent is the tendency to distinguish sharply between a proposition and a sentence and to consider the form with which philosophical logic is concerned as a property of a proposition. This tendency finds expression both in text-books on logic—and not only in such standard texts as Eaton's *General Logic*, Cohen and Nagel's *Introduction to Logic and Scientific Method*, and Stebbing's *Modern Introduction to Logic*, but also in more recent

[1] See, for example, B. Russell, *Our Knowledge of the External World* (2nd edn.; New York: Norton, 1929), pp. 46-47. The term " philo- sophical logic " is used to refer to the basic aspects of mathematical logic. See Russell, *op. cit.*, p. 44 and W. V. Quine, *Mathematical Logic* (New York: Norton, 1940), p. 127 for the distinction underlying this usage.

works such as *Fundamentals of Symbolic Logic* by Ambrose and Lazerowitz—and more sophisticated contributions to the field of philosophical logic.[1] But in spite of the emphasis placed by so many philosophers on the distinction between propositions and sentences, there are some writers on logic, *e.g.* Quine and Tarski, who have dropped completely the term " proposition " and talk explicitly about sentences or statements, meaning by "statement " any sentence which is either true or false. These writers, as consideration of their texts [2] will evidence, still concern themselves with logical form.

Thus we see that there is a point of agreement and a point of difference among writers on logic. Nearly all agree that logic, at least in its beginnings, is concerned with form. But until recently the tendency was to distinguish between sentences and propositions, and to argue that the forms which interest the logicians, in distinction from those which might interest the poet or the grammarian, belonged to propositions, not to sentences. To-day, however, some logicians consider logical form to be a property of sentences. They have found the concept of proposition entirely unnecessary, and in fact, have avoided the term " proposition " in the exposition of philosophical logic as if its use were misleading. This disagreement defines a problem, namely that of determining which of the two points of view is correct. To decide this issue in favour of those who maintain that sentences are the bearers of the forms which are the subject matter of philosophical logic—which is an aim of this paper—one could proceed in at least two distinct ways. On the one hand there is the alternative of an *ad hominem* argument against each philosopher who holds that it is essential to distinguish between sentences and propositions and that logical analysis is concerned with discovering the forms of the latter. On the other hand, one could attempt to state clearly what he means by " logical form of a sentence " and how he determines what is the logical form of a sentence. I will deny myself the pleasure of following the first alternative, for it could prove neither one side incorrect nor the other side correct. Whereas, on the other hand, if the second alternative can be carried out successfully, then one can judge easily whether one engaged in philosophical logic needs to be

[1] See, for example, A. P. Ushenko, " Russell's Critique of Empiricism ", in P. A. Schilpp, ed., *The Philosophy of Bertrand Russell* (Evanston : Northwestern University, 1944), p. 392, and G. E. Moore, " A Reply to My Critics " in P. A. Schilpp, ed., *The Philosophy of G. E. Moore* (Evanston : Northwestern University, 1942), p. 661.

[2] A. Tarski, *Introduction to Logic* (New York : Oxford University Press, 1941) or Quine, *op. cit.*

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concerned with any thing other than sentences. Thus, let us turn to the task of carrying out the second alternative.

2. *Explication of "logical form"*

By limiting the discussion to sentences one gains an immediate advantage over those who speak of propositions, for he can use the term "form" in a literal sense. The form of a sentence is a visually discernible structural property of that sentence. Consider, for instance, the classical example :

(1) If every man is mortal and Socrates is a man then Socrates is mortal.

If we now construct a schema by putting an "X" wherever "man" occurs, a "Y" wherever "mortal" occurs, and a "Z" wherever "Socrates" occurs in (1), we depict the form of (1). Thus,

(2) If every X is Y and Z is a X then Z is Y

represents the form of (1). This example illustrates the usual form-content distinction. (2) represents the form of (1); the terms which occur in (1) in the places where the letters "X", "Y" and "Z" occur in (2) constitute the matter, *i.e.* the content, of (1). To say of (1), as is often done, that it is true in virtue of its form means that every sentence which can be constructed from (2) by uniform replacements of the letters "X", "Y" and "Z" is true.

This example, however, can be considered only a first approximation toward explication of what is meant by "the logical form of a sentence". Other examples lead to some difficulties. Consider, for example

(3) Dogs bark

and

(4) Unicorns exist.

In (3) and (4) we have no "little words"; at best we could represent the form of these sentences by two letters, that is

(5) X Y.

But few philosophers since Hume have denied that (3) and (4) differ in logical form, and Russell has repeatedly insisted that these sentences differ in logical form. In fact, for Russell, the identity of grammatical structure disguises their real logical form. Likewise, with sentences such as

(6) The whale is a mammal

and

(7) The author of Waverley is a poet.

If one disregards the little word " of ", and treats " author of Waverley " as one term, then both (6) and (7) have the form :

(8) The X is a Y.

But again, most philosophers follow Russell in admitting that although there is a structural similarity between (6) and (7), such a similarity merely hides the fact that they have different logical forms.

Now, if we call the discernible visual structure of a sentence its " grammatical form ", then we could distinguish the logical form of a sentence from its grammatical form by stating that the logical form is the grammatical form the sentence *would have* in an ideal language. Thus, as a second approximation to a definition of " logical form " we state that the logical form of a sentence is the grammatical form the sentence should have. Such an understanding of logical form indicates one of the senses in which logic is a normative science. One has often heard it stated that logic tells us how we ought to think; it also tells us what grammatical forms sentences ought to have.

Terms such as " ideal ", " should ", and " ought ", however, do not convey much, if any, information unless a purpose is stated. One is justified in asking, " Ideal for what purpose ? ", " Should have for what purpose ? ". And to these questions there is a direct and simple answer : Ideal for indicating the logical relations that sentences have to each other. This does not mean that in an ideal language one would be able in every case to determine by immediate inspection the logical relation of two sentences. But the structure of the sentences would be such that a definite procedure, usually called a " derivation ", for discovering the logical relation of two sentences could be described. Such a procedure would not guarantee that one would always be able to answer the question : " What is the logical relation of sentence A to sentence B ? "; but it would practically preclude giving the wrong answer. An ideal language, then, is one in which the grammatical structure of sentences is such that the symbolism does not suggest false beliefs, spurious arguments or invalid inferences.

The suggestion that the logical form of a sentence is the grammatical form that it ought to have, if correct, indicates why many philosophers have thought that something other than the sentence had the logical form. The grammatical form is given ; it can be observed. The logical form, on the other hand, is somewhere behind this grammatical form, in many cases hidden by it. Hence, there must be some other object which has this form,

and it is necessary to look behind the sentence to this other object in order to discover the forms which logic seeks. Thus, such philosophers distinguish between language and some non-linguistic realm which language expresses or refers to, instead of making the distinction between a relatively imperfect language and a relatively perfected one.

If the logical form of a sentence is the grammatical form that it ought to have in order to exhibit clearly its logical relations to other sentences, then it is clear how one determines what is the logical form of a sentence. To determine the logical form of a sentence one must investigate that sentence in relation to other sentences to see what sentences it implies and what sentences imply it. On first consideration such a view may appear circular. It might stimulate a votary of the proposition cult to argue something like this : You state that the logical form of a sentence is to indicate the logical relations of that sentence to other sentences, but in order to discover the logical form of a sentence one must already know its logical relations to other sentences. This is paradoxical, and to avoid such a paradox one must go behind sentences to the propositions that they mean. The propositions are there with the forms ; they have logical relations to other propositions, which relations are indicated by their form. Our job as logicians is to discern these propositions and their forms, and then put the sentences which express them into a symbolism such that the structure of the sentence reflects the structure of the proposition. It is absurd to assume that one can discover the logical form of a sentence by examining sentences when by hypothesis sentences do not have logical form.

The spurious character of our propositionalist's argument becomes evident once we state our position more fully. We take one assumption : sentences do have logical properties, that is, for example, some sentences imply other sentences ; some sentences contradict other sentences. But the sentences of the ordinary language do not indicate by their structure the logical relations they have to one another. For example, are the two sentences

(9) The present king of France is bald

and

(10) The present king of France is not bald

the contradictory the one of the other ? Their form suggests that they are. Russell has argued that they are not ; Frege would argue that they are. An ideal language, in our sense of the term "ideal", would be one in which the translations of (9) and

(10) would indicate clearly the logical relation of (9) and (10).
We will return to this example in more detail later.   Consider
further the classical examples

> (11) All men are mortal
> (12) No men are mortal
> (13) All men are not mortal
> (14) Some men are not mortal
> (15) Not all men are mortal.

Which pairs, if any, of these five sentences are contradictories ?
The layman who ordinarily uses sentences with such forms will
have a difficult time answering. Given the translation of these
sentences into an ideal language, their forms will indicate clearly
the correct answer.

These examples illustrate that although sentences do have
logical relations one to another, their grammatical structure does
not, in all cases, indicate clearly—that is, make apparent to
immediate inspection, or allow for the application of a definite
procedure for transforming a sentence from one form to another—
what these logical relations are.   Now the logician, once he has
decided what the logical relations of a sentence are, wants to
translate that sentence into an ideal language in such a manner
that its structure in this ideal language indicates these logical
relations.   In other words, in an ideal language the grammatical
structure of the sentence would be a reliable indicator of its
logical properties ; thus, in this ideal language, one calls the
grammatical structure of the sentence, the " logical form " of
the sentence.   In view of these considerations, we can now
amend our second approximation towards an explication of
" logical form ".   Strictly speaking, the logical form of a sentence
is not the form *it* ought to have but the form that *its translation*
in an ideal language ought to have.   And as in the case of any
translation, one must investigate the uses and properties of the
sentence to be translated in order to make the translation.

3. *Examples of investigations of logical form*
Introduction

There are at least three factors which make the logician's
task of discovering logical forms extremely difficult. Firstly,
there is the problem of determining what in fact are the logical
relations of sentences in the ordinary language.   In a sense, this
is just the question of determining what persons mean by sen-
tences they use.   Various methods are available for determining
what a person means by a sentence, but there is no guarantee

that one's efforts will be completely successful. Secondly, there is the fact that different logicians have different opinions as to the basic terms in an ideal language. In most cases a disagreement as to form on this ground is not significant, for sentences can be translated from one ideal language to another as well as from the ordinary language into an ideal one. However, one logician's ideal language may indicate the logical relations of sentences better than another's, and on this point there is ground for significant argument. And thirdly, most logicians are also philosophers, and hence have in mind, either explicitly or tacitly, criteria of excellence other than merely that of indicating the logical relations of sentences by their form. Such additional criteria will affect the translations.

It is beyond the scope of this paper to go into any of these points in any detail. Instead, I shall illustrate the preceding explication of " logical form " by an examination of some disagreements between Russell and Carnap concerning logical form which are set forth in Carnap's book *Meaning and Necessity*.[1] I believe that an examination of these disagreements is not only illustrative of the above explication, but interesting in its own right.

(A) *Frege and Russell on Definite Descriptions*

Russell's theory of descriptions is too familiar to require an exposition. According to that theory the logical form of a statement such as

(16) The author of Waverley is a poet

is depicted by the schema

$$(17) \ (Ey) . fx \equiv_x x = y . gy.$$

Carnap, following Frege, suggests

$$(18) \ (Ey) . fx \equiv_x x = y . gy : v : - (Ey) . fx \equiv_x x = y : ga*$$

as the schema which depicts the form of (16) and similar sentences. In this schema " $a*$ " is the name of any arbitrarily chosen individual.

Is one justified in assuming that one of these two recommendations as to the logical form of (16) is correct and the other incorrect ? The preceding explication of what is meant by " logical form " makes it clear that such an assumption is not justified. However, there are considerations which could

[1] Rudolf Carnap, *Meaning and Necessity* (Chicago : University of Chicago Press, 1947). Throughout the rest of this paper, reference to Frege's point of view is an indirect way of referring to Carnap's formulation of Frege's point of view.

lead one to reject one or both of these recommendations. Let us turn now to such considerations.

The logical form of a sentence, that is, the structure of its translation in an ideal language, is determined in part by the uses of the sentence in the ordinary language. Let us consider first the differences that might occur in respect to ordinary usage given these two different recommendations. Given Russell's understanding of the logical form of sentences containing the term " the " when it is used in the singular, we are forced to conclude that we cannot infer from the law of identity

(19) The winged horse captured by Bellerophon is identical with the winged horse captured by Bellerophon.

At first glance, this seems at odds with common usage. On the other hand, given Frege's understanding of the logical form of sentences containing " the " in the singular, not only does (19) follow from the principle of identity, but the sentence

(20) Pegasus is Medusa

follows from the true sentence

(21) Neither Pegasus nor Medusa exists.

This last result seems more at odds with common sense than the rejection of (19) as a specification instance of the law of identity. Further, for Russell, the sentence

(22) The present king of France is not bald

implies

(23) It is not the case that the present king of France is bald.

But the converse implication does not hold. This, too, at first sight, seems strange. For Frege, however, (22) and (23) are, as one would expect, mutually implicative.

Sentences in an ideal language should be such that the structure of each indicates its logical relations to other sentences. This aim is furthered if the logical forms are such that the rules of derivation are relatively simple and their applicability a function of the forms alone. The theorems of logic become the rules of derivation for a language. Now from the point of view of simplicity of derivation, Frege's analysis has the advantage over Russell's in that specification and particularization can be applied to sentences containing descriptive phrases as well as to sentences containing names. In view of the fact that most so-called " names " are disguised descriptions, this is a great advantage. Further, given Frege's analysis, one does not find, at least for extensional contexts, the distinction of scope necessary.

The logical structure of a sentence should indicate clearly whether or not the sentence is analytic, that is, is a substitution-instance of a theorem of logic. Consider for example, the sentence

(24) The man who lives at the north pole lives at the north pole.

This sentence seems to be a tautology (in the non-technical sense of "tautology"). But given Russell's analysis it is a genuine statement of fact. (24) is true if and only if one and only one man lives at the north pole. The sentence (24) is just one of the many cases in which Russell must prefix an hypothesis to the effect that the described entity exists in order to secure an analytic sentence. Frege's system, however, is no better than Russell's in this respect. (24) is not a substitution instance of a theorem of logic on his analysis either. For Frege two factual considerations are relevant to the truth value of (24), namely, the existence of the described entity and where a^* lives. And this is not the only case of its kind in Frege's system. The sentence

(25) Anyone identical with the present king of France is a king of France

according to Russell's analysis is analytic. But such a sentence is not a substitution instance of a theorem of logic for Frege. It is true, however, whenever the unidentified person in question is not the arbitrarily chosen person a^*.

These differences between Frege's and Russell's analysis of the logical structure of sentences containing "the" in the singular are mainly a result of a difference of opinion concerning the alleged denotation of descriptive phrases. Frege believed that in an ideal language every name (description or proper name) must denote something. Russell, on the other hand, decided, in fact, thought he had proved, that no descriptive phrase ever denotes anything.

It is not our purpose here to decide between Frege's and Russell's suggestions. We merely wanted to illustrate by this example at least three different reasons why two philosophers might arrive at different decisions concerning the logical structure of sentences. There is, first, the actual use of the sentences in question in the ordinary language. There are also considerations concerning the structure of the ideal language itself; and finally, there are philosophical attitudes concerning the nature of an ideal language. Matters of usage can never be decided definitively, and there is no reason to assume that usage is consistent to a degree which will allow complete systematization.

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Matters of convenience within a deductive system are matters
of convenience, and there is no reason to assume that a consensus
as to what is most convenient will be reached.   Philosophical
theories of language, like most philosophical theories, are such
that between any two of them it is hard to find any theoretical
difference.

B. *Carnap and Russell on Indefinite Descriptions*

In contrast to the case of Frege (Carnap) *v.* Russell concerning
the analysis of sentences containing the term " the " in the singu-
lar, is the case of Carnap *v.* Russell concerning the analysis of
sentences containing " the " in the plural.   I believe that one
cannot find any crucial argument for deciding the former case in
favour of either point of view.   It was for this reason that the
example was chosen.   On the other hand, in the latter case, I
believe that one can find serious objections to at least one, and
maybe to both, of the points of view expressed.   Let us turn,
now, to a consideration of this latter case.

In order to facilitate the development of this illustration
Carnap's terminology will be employed.   We begin by stating
the necessary and sufficient conditions for the truth of any
sentence

$$(26) \quad \ulcorner Np \urcorner$$

namely,

$$(27) \quad p \text{ is L-true.}$$

We shall employ Russell's notation for class and property
expressions, but—following Carnap—we shall dispense with the
notion of a predicative function.   Identity of properties, then,
can be indicated in the following manner :

$$(28) \quad f\hat{x} = g\hat{x} \ . \ \equiv \ . \ \text{N}(fx \equiv_x gx).$$

Now, let us consider an ideal language, which we shall call
" PM' ", which consists of the language of *Principia Mathematica*
supplemented by descriptive constants.   Given that " H " is a
descriptive constant of PM', then the following is a true sentence :

$$(29) \quad \text{H}x \equiv_x \text{H}x \ . \ \text{N}(\text{H}x \equiv_x \text{H}x).$$

From (29) and (28) by replacement one can secure

$$(30) \quad \text{H}x \equiv_x \text{H}x \ . \ \text{H}\hat{x} = \text{H}\hat{x}.$$

And from (30) by particularization, one secures

$$(31) \quad (\text{E}g) \ . \ gx \equiv_x \text{H}x \ . \ gx = \text{H}\hat{x}.$$

Let us assume further that PM' contains a predicate " F " such
that the following sentence is true.

$$(32) \quad \text{F}x \equiv_x \text{H}x \ . \ - \ \text{N}(\text{F}x \equiv_x \text{H}x).$$

By applying transposition to (28) and using the result to make a replacement in (32), and then utilizing particularization in connexion with what results from this replacement, one secures

$$(33) \ (Eg) \cdot gx \equiv {}_xHx \cdot g\hat{x} \neq H\hat{x}.$$

These results do not seem at all puzzling ; one could understand (31) to state :

(34) There is some property equivalent to $H\hat{x}$ which is also identical with it,

and understand (33) to state :

(35) There is some property equivalent to $H\hat{x}$ which is not also identical with it.

But the sentences (31) and (33) can be abbreviated in PM′, and these abbreviations are somewhat misleading. The sentence (31) can be abbreviated as :

$$(36) \ \hat{z}(Hz) = H\hat{z}$$

and (33) can be abbreviated as :

$$(37) \ \hat{z}(Hz) \neq H\hat{z}.$$

The sentences (36) and (37) appear to contradict one another ; they suggest—reading " H " as " man "—the following interpretations respectively :

(38) The class of men is identical with the property man.

(39) The class of men is not identical with the property man.

The abbreviations (36) and (37) are secured by means of the following definition :

(40) $\ulcorner \ldots \hat{z}(fz) \ldots \urcorner$ for $\ulcorner(Eg) \cdot gx \equiv {}_xfx \cdot \ldots g\hat{x} \ldots \urcorner$

Such a definition, from our point of view, can be considered a rule for translation ; the left-hand side indicates the grammatical form of a sentence in the ordinary language ; the right-hand side the grammatical form of the translation in the ideal language, *i.e.* the logical form of the original sentence. That is to say, (38) and (39) are sentences in the ordinary language ; (34) and (35) their translations in the ideal language. Carnap's criticism, from our perspective, is this : Is an analysis which translates sentences which are apparently contradictories into sentences which are not contradictories satisfactory ; that is, is (40), which leads to these results, a satisfactory analysis of the logical form of the sentences in question ? In other words, if Russell's analysis is correct, then we must accept both (38) and (39) as true, and this is not in accord with our ordinary understanding of such sentences. To avoid this difficulty, Carnap offers the following definition in place of (40) :

(41) $\ulcorner \ldots \hat{z}(fz) \ldots \urcorner$ for $\ulcorner(g) : gx \equiv {}_xfx \cdot \supset \cdot \ldots g\hat{x} \ldots \urcorner$

But Carnap is forced to admit that his suggestion also has some
not too desired results.   Given Carnap's definition, (38) and (39)
translate respectively :

$$(42) \ (g) : gx \equiv {}_xHx . \supset . g\hat{x} = H\hat{x}$$
$$(43) \ (g) : gx \equiv {}_xHx . \supset . g\hat{x} \neq H\hat{x}$$

Neither (42) nor (43) can be established as true on the conditions
given.   Hence, although Russell's suggestion seems to violate
the principle of contradiction, namely, that we can establish
both a sentence and its negation as true, Carnap's suggestion
seems to violate the principle of excluded middle, that is, we
cannot establish either a sentence or its negation as true.   Con-
cerning his proposal, Carnap states :

> this apparently, but not actually, violates the principle of excluded
> middle ;   however, this seems less disturbing than the previous
> apparent violation of the principle of contradiction.[1]

Unless one has an extreme distaste for apparent contra-
dictions and a strong penchant for violating apparent middles,
he can find little cogency to this argument.   In fact, Russell's
analysis here is no better or worse in this respect than his analysis
of sentences containing " the " in the singular.   Once one takes
account of what Russell calls " scope ", then the sentence :

(44) It is not the case that the class of men is identical with the
property man

is not equivalent to (39) and is the legitimate contradictory
of (38).   But not both (38) and (44), which translates

(45) It is not the case that there is some property which is
equivalent to $H\hat{x}$ and identical with it

are true.   Thus, we find here no serious defect in Russell's
analysis.

On the other hand, Carnap's suggested analysis does suffer from
a serious defect.   Given the point of view being maintained
in this paper, an essential characteristic of the logical form of a
sentence is that it indicates the accepted logical properties of the
sentence.   Carnap's rule for translation lacks this characteristic.
For given his analysis, a sentence asserting the formal equivalence
of two properties does not have as a logical consequence the
sentence asserting the identity of the classes defined by these
properties.   That is to say, given (41) as our rule of translation,
one cannot establish

$$(46) \ fx \equiv {}_xgx . \supset . \hat{z}(fz) = \hat{z}(gz)$$

[1] *Meaning and Necessity*, p. 150.

as a theorem of logic.   For assume that (46) is a theorem, then
by Carnap's definition, namely (41), we secure

(47) $\hat{z}(fz) = \hat{z}(gz)$. $\equiv$: . $(h)\,(k) : hx \equiv {}_xfx$. $kx \equiv {}_xgx$. $\supset$. $h\hat{z} = k\hat{z}$

and from (46) and (47)

(48) $fx \equiv {}_xgx$. $\supset$: . $(h)\,(k) : hx \equiv {}_xfx$. $kx \equiv {}_xgx$. $\supset$. $h\hat{z} = k\hat{z}$

follows ; from (48) by specification we have

(49) $fx \equiv {}_xgx$. $\supset$: $gx \equiv {}_xfx$. $fx \equiv {}_xgx$. $\supset$. $g\hat{z} = f\hat{z}$

which yields

$$(50)\ fx \equiv {}_xgx\ .\ \supset\ .\ g\hat{z} = f\hat{z}.$$

But (50) states that formally equivalent properties are identical,
which is not the case.   Russell usually cites the properties of
being human and being featherless bipedal as a contrary instance.
Therefore, (46) is not a theorem, which is to say, for example,
from

(51) (x) (x is a man if and only if x is a featherless biped)

one cannot infer (validly) the sentence

(52) The class of men is identical with the class of featherless
     bipeds.

The failure of this inference to hold is an extreme divergence from
our usual understanding of the sentences involved, and is sufficient
grounds for rejecting Carnap's suggested rule of translation.

There is a respect, however, in which Carnap's definition seems
definitely preferable to Russell's.   Consider the matrix

$$(53)\ fx \equiv {}_xfx\ .\ f\hat{x} = f\hat{x}$$

from which by particularization and generalization, we can infer

$$(54)\ (f)\,(\mathrm{E}g)\ .\ gx \equiv {}_xfx\ .\ g\hat{x} = f\hat{x}.$$

The sentence (54) abbreviates

$$(55)\ (f)\ .\ \hat{z}(fz) = f\hat{z}$$

which is read

(56) Every class is identical with its property.

The terms " class " and " property " have no well established
use, and there are many sentences containing them, such as (56),
whose literal meaning is difficult to state.   However, I think
that we do understand these words in such a manner that we
would want to reject (56) ; that is, consider it false ; in fact, our
usual understanding of these terms is such that we would want
to assert that

(57) No class is its property

is not only true, but provable.   Given Carnap's suggested
definition (56), as we have seen, is not provable ; but (57),

on the other hand, can be deduced. This indicates that with respect to non-existensional contexts, Carnap's suggestion is a better analysis than Russell's

Our statement that (57), understood according to Carnap's definition, can be proved needs some elucidations and qualifications. Let us begin with the elucidations. Roughly, we would say that one could identify a class and a property in case no two properties defined that class. That is, we do not want to identify the class of men with the property man, for this would force us to identify the property featherless biped with the property man. This latter identification would be a mistake. If we follow Carnap and take equivalence of predicators as the condition for the identity of the classes which are their extensions, and L-equivalence of predicators as the condition for the identity of the properties which are their intensions, then we can formulate the conditions for the identity of a class with its property in the following manner :

$$\text{(58) } (g) : gx \equiv {}_x\mathrm{H}x \, . \, \supset \, . \, \mathrm{N}(gx \equiv {}_x\mathrm{H}x).$$

By the convention (28), this becomes

$$\text{(59) } (g) : gx \equiv {}_x\mathrm{H}x \, . \, \supset \, . \, g\hat{x} = \mathrm{H}\hat{x}.$$

The sentence (59), according to Carnap, asserts

(60) The class $\hat{z}(\mathrm{H}z)$ is identical with the property $\mathrm{H}\hat{z}$.

To get the general assertion that we desire, namely (57), we merely negate (59) and then generalize, giving :

$$\text{(61) } (f) - [(g) : gx \equiv {}_x fx \, . \, \supset \, . \, g\hat{x} = f\hat{x}].$$

And by opposition of quantifiers, (61) becomes

$$\text{(62) } (f) \, (\mathrm{E}g) - [gx \equiv {}_x fx \, . \, \supset \, . \, g\hat{x} = f\hat{x}]$$

a proof of which follows.

Let " H " be an empirical predicate and " s " be an individual constant. Now consider the following disjunction, which we will call " (I) ".

    (i) $- [fx \, . \, \equiv_x . fx \, . \, \mathrm{H}s : \supset : \mathrm{N}(fx \, . \, \equiv_x . fx \, . \, \mathrm{H}s)] \, . \, v.$

    (ii) $- [fx \, . \, \equiv_x . fx \, . \, - \mathrm{H}s : \supset : \mathrm{N}(fx \, . \, \equiv_x . fx \, . \, - \mathrm{H}s)] \, . \, v.$

    (iii) $- [fx \, . \, \equiv_x . fx \, v \, \mathrm{H}s : \supset \ : \mathrm{N}(fx \, . \, \equiv_x . fx \, v \, \mathrm{H}s)] \, . \, v.$

    (iv) $- [fx \, . \, \equiv_x . fx \, v \, - \mathrm{H}s : \supset \ : \mathrm{N}(fx \, . \, \equiv_x . fx \, v \, - \mathrm{H}s)].$

If f is empirical or L-determinate true, then either (i) or (ii) must be true ; for the consequent of each is false, but the antecedent of at least one must be true. If f is empirical or L-determinate false, then either (iii) or (iv) must be true ; for the consequent of each is false, but the antecedent of at least one must be true.

Hence, regardless of what type of predicate f is, (I) is true. But since each of (i) through (iv) implies

$$(63) \quad (f)\,(\mathrm{E}g) - [fx \equiv_x gx . \supset . \mathrm{N}(fx \equiv_x gx)]$$

(I) implies (63). By reference back to (28) we see that (63) is equivalent to (62), hence, (I) implies (62), which concludes our proof.

The statement, that by deducing (62) we have proved (57), namely, that no property is its class, needs the following qualifications. Firstly, although the statement does not appear to make any reference to a language, if we trace back the definitions involved in formulating the statement, we see that the statement is relative to a language. The term which connects the sentence with a language is " L-true ". This term is defined by Carnap relative to a language. Thus, we have not proved that no class is its property, but merely that no class is its property with respect to a certain language. Secondly, the language to which the statement in question is relative must be of a specific nature. Namely, it must contain at least one predicate which is not L-determinate. The need for this assumption is obvious in the proof. But given these two qualifications, then, on Carnap's suggested definition we can prove that no class is its property.

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V.—DISCUSSIONS

A NOTE ON TRUTH

MR. P. F. STRAWSON, in his paper on " Truth " in *Analysis* for June 1949, and again in his contribution to the symposium on the same subject at the 1950 Joint Session, has argued that the words " true " and " false " are (in effect) performatory rather than descriptive words, and hence that the sentences in which they appear express activities rather than state facts. The original purpose of his argument was to confute the semanticist view that truth is a property of symbols ; but the interest of his thesis is much wider. For if Mr. Strawson is right, not only does the Semantic Theory have to go, but also all the classical theories of truth, whether of the Coherence or the Correspondence variety. These theories sought to identify the relation of truth and to say between what kinds of thing it held ; but if " true " and " false " are not the names of relations (or indeed of anything), these problems do not arise.

I want to try to show very briefly that there is an omission in Mr. Strawson's account which is fatal to the destructive side of his case.

Mr. Strawson arrives at his view of the performatory function of the words " true " and " false " by examining our common usage of such expressions as " That's true " and " It is true that. . . ." He shows that our purpose in using such expressions is (according to circumstances) to endorse, underline, confirm or concede what has been or might be said by another. That it is part of our purpose, often a prominent part of it, I have no wish to deny. But whether this account covers all aspects of our everyday usage of " true " and " false " is another matter, and it is here that I find Mr. Strawson's theory defective.

Consider the following possible conversation between three speakers :

A : " X has been appointed Professor ".
B : " I don't believe it ".
C : " Oh yes, he has ; it's perfectly true ".

Does C here merely confirm, endorse or underline what A has said ? Following Mr. Strawson, we might well be inclined to say that is all there is to it ; but suppose the conversation continues :

B : " Why do you say that ? "
C : " Because it's the truth " (or " Because it *is* true ").

Does C in his second statement continue to do no more than endorse what A originally asserted ? This is what I find hard to believe ;

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for if it were the case, the second part of the conversation could be paraphrased thus :

B : " Why do you endorse what A says ? "
C : " Because I endorse it ".

That this paraphrase is inadequate seems to me perfectly obvious, and the reason for its inadequacy can be simply indicated. C, when he speaks for the second time at any rate, is not only endorsing A's original statement, as Mr. Strawson points out, but also doing something more, namely saying that what A said is *acceptable* or *fit to be endorsed*. And this is a general feature of our use of the word " true ". To say that *p* is true is not merely to assert *p* but further to say that *p* ought to be asserted ; it is to imply that there are grounds for asserting *p* or that there is evidence for it which is generally valid. Any theory of " true " which neglects this aspect of the word—which fails to allow for the implication that what is true is valid not merely for one person but for all—is leaving half the story out ; and incidentally it was with this half that the classical theorists of truth were concerned.

Perhaps I can make my point clearer, and show that " true " and " false " are different in an important respect from other words of a performatory nature, by a different kind of approach. Let us imagine two parallel dialogues, one containing a genuine performatory expression, the other the word " true ". The first might run as follows :

A : " I name this ship *Emily* ".
B : " Why do you say that ? "
A : " Because I do name her *Emily* ".

Here question and answer alike have an air of the absurd, and the reason is that A's original utterance was not, in the strict sense, a statement at all. But would the same be true if we substituted :

A : " What he says is true ".
B : " Why do you say that ? "
A : " Because it *is* true " ?

For my part I cannot see that it would. I can, of course, see that we might well find A's part in the conversation irritating if we were B, just as, if we were A, we might find B's question annoying or inconvenient. But it is one thing to talk in an irritating way, and another to talk nonsense.

The defects of the performatory theory, if there is anything in my argument, lie not in what it asserts but in what it denies. When we use the word " true " (and again when we use the word " false ") we are, if this sort of language is preferred, evincing an attitude ; a point which was grasped, though in an imperfect way, by old-fashioned writers in their talk of " judgment ". But though this is true, it is not the whole of what we are doing. For the word " true ",

like the word " good ", has (to use a dangerous antithesis) an objective as well as a subjective side to it : at the least we intend to say, when we apply it to a statement, that that statement ought to be accepted (not merely to indicate that we are accepting it).   It seems to me that this side of the matter is almost entirely overlooked by Mr. Strawson. He does say on page 95 of his *Analysis* article that " the linguistic performance (of ' confirming ') requires . . . an *occasion* " ;   and he does recognise that we can properly ask *when* we are justified in going through the motions of endorsing, underlining, conceding, etc.   But he seems to think such questions fall outside the proper province of a theory of truth ; whereas I should hold that they are central to it.   If the older theorists did scant justice to those aspects of our use of " true " and " false " to which Mr. Strawson calls attention, it is no adequate remedy to fly to the other extreme and claim that these aspects exhaust the whole content of the terms. For this would seem to be patently false.

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That two or more impressions can be definitely simultaneous in the experience of a single mind is scarcely open to question. Can we make a similar assertion regarding impressions on two separate minds ? Is such a question meaningless ?

TIME ORDER FOR MINDS

THE Relativity challenge to the concept of physical simultaneity has prevailed ; but, although the " Einstein effect " frequently enters into astronomical and sub-atomic calculations, the paradox (for such it still seems to common sense) of variable dating of events has not seriously obtruded itself in ordinary affairs. Nevertheless, one may pause to ask if the strange principle involved has been fully digested, or its application adequately recognised, in the context of philosophical psychology. Memory will readily recall Eddington's imaginary traveller who could move through space at a velocity which, by slowing down his watch and (presumably) his physiological processes in comparison with an earthly contemporary, could expand his lifetime prodigiously and totally subvert co-incidences of dates as between himself and friends still using the terrestrial calendar. Although human limitations protect us from such glaring discrepancies we cannot ignore the social significance of simultaneity of conscious experience, and I should like to revert to this topic which I raised over twenty years ago in *The Australasian Journal of Psychology and Philosophy* (Sept., 1930). May I quote briefly :

> There is an intense differentiation between our attitude towards what we believe, for example, to be *present* suffering and suffering in remote ages. It would be difficult to believe that a sigh of relief elicited when an untoward experience of a friend is, as we say, over and done with, has no sort of justification other than that founded on our peculiar point of view. We may, of course, be utterly deceived in imagining that there is such a thing as a veritable contemporaneousness of minds, because the rational foundation of the idea seems lacking. But taking the almost irresistible intuitive view of its reality, it must find its basis, not in physical expression, which can yield us only the relative and variable, but in a detached realm of personality and non-metric experience.

Notwithstanding J. S. Mill and his followers, I think it should now be widely recognised that the laws of Nature are not final and objective invariants, but rather are more or less efficient devices for the survey of the physical world. There may be, and are, optional systems—as with the Ptolemaic, Copernican and Einstein conceptions. The simplest system prevails, but we have no guarantee that it is final, or even that on a wider view it is plausible. A

solipsist, if he were a sufficiently able mathematician, might well devise an egocentric physical universe. Similarly, if one law be fixed and adopted the remaining laws, however complex, might be brought into line, just as an appropriate breadth could be assigned to any arbitrary length to produce a given area. Einstein's system deprives rigid rods, and indeed rigid frameworks of any sort, of universal validity, and physical science agrees because thereby the common objective realm becomes more consistently amenable to elementary principles. We forego any qualms based upon reluctance to assimilate obvious solidity with spineless " molluscs "—to use Einstein's term. The case with minds conditioned by physical brains is, however, somewhat different, and one is constrained to ask if any physical theory (by no means guaranteed as final) is to be allowed to subvert instinctive apprehensions and the essence of social relations. Professor E. A. Milne in his kinematical relativity has subjected time to even greater contortions than Einstein, and Professor Dingle, while not challenging Milne's mathematics, protests against the implied immunity from impending disaster of a man standing in front of a rapidly approaching bus. Adaptable mathematics may shield this man by requiring his time system to stretch to a degree which will radically postpone the threatened impact, but common sense will scarcely allow that history as we know it can thus be stalled. Of course, a behaviourist, who sees nothing in minds but physical processes, may well be content to allow the time gauge to shrink or expand indefinitely, conscious duration having little or no claim with him, but that surely does violence to experience. Even in classical physics optional systems of law cannot be excluded, just as in geometry three points can prescribe either a triangle or a circle. If an infinite velocity of light be adopted no doubt the physical system would become outrageously involved ; but, after all, has Nature any care to be simple ? If we could view directly the daily life of inhabitants of a nebula at, say, a million light years distance, present theory would decide that we were looking back a million years in history. On an infinite velocity theory the people would be our true contemporaries. Surely there is a difference between these two interpretations altogether aloof from physical theory. A realistic philosophy of common sense might well accord to directly observed fellow beings a sympathetic recognition as contemporaries rather than as a faint reproduction of an age-old drama. If it be objected that a two million year lapse would intervene before response to a signal, that is *in principle* no different from the fraction of a second which must elapse in our closest and most intimate exchanges. In either case, moreover, the interval is a function of a *selected* time system. The gist of my concern is to ask if philosophical psychologists have not some right to postulate simultaneities and successions not indissolubly tied to the chariot wheels of physical theory. One is fully aware that the modern logical empiricist will deny meaning

to such a conception, but it is just one phase of the problem of other minds, which is insuperable in a physicalist theory. It marks the limitations of the logical empiricist position in the field of general philosophy. If there be not an absolute significance in simultaneity of thought and feeling beyond the experienced simultaneities of a single mind, and transcending also the shifting criteria of advancing science, an essential of social life would seem to be in danger of stultification. The problem is not immediately clamant, but an interesting principle is involved. We have always acquiesced in a time system which often does violence to our inner sense—clock-time, in certain circumstances, seeming quite out of step with our conscious intervals—but is it not asking rather much that we should tolerate a jumbled medley of present, past and future? Einsteinian Relativity may not invade ordinary life very drastically—Eddington at least concedes us an "absolute elsewhere" and absolute elsewhen—but in view of such systems as Kinematical Relativity there seems little guarantee that physics of the future will not abolish even these reservations.

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THE INCONGRUITY OF COUNTERPARTS

" IF I look in a mirror the left side of my face will appear as the right side of my face." Why is this sentence necessarily true ?

This question raises Kant's problem of the incongruity of counterparts. Wittgenstein's answer was " Because space has no more than three dimensions." Now this answer is not quite complete, since it leaves a small residual problem. It does not explain why left and right behave like this while top and bottom do not. Is it perhaps because we have two eyes, or because of the way light is reflected ? It is easy to see that the correct explanation is neither of these : indeed that it is not physical, but logical. Nor is it difficult to provide this logical explanation. Nevertheless it is worth while elaborating it in its full detail, since the sentence happens to illustrate a certain type of puzzle in a remarkably clear way. On the one hand it is necessarily true : and yet on the other hand it does not seem quite right to say that it owes its necessary truth to the definitions of ' left ' and ' right '. My solution of this puzzle will be that it does indeed owe its necessary truth to the definitions of ' left ' and ' right ' : but that certain unnoticed contingent facts make these definitions appear superfluous ; and that this appearance is a delusion.

Assuming, then, that space is three-dimensional, and not inquiring into the nature of this assumption, I ask what is the additional source of the necessary truth of the sentence about the reflection of my face. Certainly there is no difficulty about the optical facts. I know where my nose, chin, left eye and right eye appear on the surface of the mirror, and I know why they appear where they do. So, in order to eliminate the optical facts from this inquiry, I imagine that I look into a mirror and then produce a full-length portrait of myself accurately painted over the mirror-image. Next suppose that I go round behind the mirror and face the back of it. Suppose also that the mirror were made of some flexible plastic material. Then I could put the portrait of my face on my face like a mask, and the portrait of the rest of my body on the rest of my body like a complete suit of clothes. I shall call this process ' getting into my portrait '. Now it is clear that, if I got into my portrait, left and right would be reversed, while top and bottom would not be reversed. Yet this reversal of left and right would take place only because I turned right about in the vertical axis in order to face the back of the mirror. But suppose that instead of turning in the vertical axis I turned in the horizontal axis about which we pivot when we turn head over heels : suppose that I stood on my head behind the mirror. It is equally clear that, if I got into my portrait in this way, left and right would not be reversed while top and bottom would be reversed. Now it is a contingent fact that we normally

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turn in the vertical axis and not in the horizontal axis. Therefore,
if ' left ' simply means ' on the same side as the heart ', and ' right '
simply means ' on the opposite side to the heart ', then the sentence
is synthetic, and I can falsify it by the easy device of standing on
my head.

But it would be a simple matter to make the sentence analytic.
All that is needed is that ' left ' should be re-defined as ' on one side of
the axis of turning ', and ' right ' as ' on the other side of the axis
of turning '. For then, if I turned in the horizontal axis which runs
through my ribs, ' left ' and ' right ' would come to mean what ' top '
and ' bottom ' now mean. And so, no matter how I turned, the
sentence would remain necessarily true.

The puzzle arises because the obvious fact that we normally
turn in the vertical axis is tacitly taken for granted. This fact is so
firm that it appears superfluous to make the sentence analytic, like
gilding the lily. But this appearance is a delusion. For if the sentence
is to be necessary, it can only be because it is analytic. And the
fact that we normally turn in the vertical axis, which creates the
delusion, is a fact which looms too large and obvious for it to be
suspected of doing anything so clandestine. Hence its importance
goes unnoticed.

At this point two objections might be made. The first is that my
picture of a world in which people always turned about by standing
on their heads is absurd. And the second is that, though standing
on one's head might work for the peculiar process of getting into
one's portrait, it would not work for such an everyday process as
meeting people in the street. For, if two people met in the street in
my strange world, one would be on his feet and the other on his
head. But in a conversational group of three people either two would
have to be on their feet or two on their heads. And in either case
these two would not be facing one another (in the new sense of that
term).

This second objection emphasizes the general nature of the
problem. For it shows clearly that the problem of reflections,
when it is stripped of its optical complications, is an ordinary case
of the general problem "How can objects be turned in three-
dimensional space" ?

The answer to the second objection is that my strange world
presents no difficulties which cannot be overcome with sufficient
imagination. For in that world everybody's left side would always
be oriented in the same direction, say east ; just as in this world
everybody's head points upwards. And so the only way for two
people to face one another in the street would be for one of them
to stand on his head. And it would be easy to manage a conver-
sational group of three people. The first would stand on his feet
and lean backwards, the second would stand on his head and lean
away from the first, while the third would be suspended half-way
between them face-downwards in mid-air. After all, the effect of

gravity on our bodies is only a contingent fact. Of course, the first
objector was right in saying that this world would be absurd.
But the answer to this is that its absurdity makes it difficult for us
to picture it ; and that, just because we find it difficult to imagine
the alteration of certain contingencies of our world, we are led to
put too much weight on synthetic sentences, letting them take the
strain of a necessity which properly can be taken only by analytic
sentences.

But the fact that we normally turn in the vertical axis is not the
only fact that secretly generates the puzzle about reflections. For,
if people stood on their heads in order to get into their portraits, they
would not succeed : since, when they tried to put on their portraits
like suits of clothes, they would find that they would not fit this
way round. Therefore, in a world in which top and bottom were
reversed in mirrors while left and right were not reversed one more
of this world's contingencies would have to be altered : people
would have to be symmetrical above and below the waist.

So the two contingent facts which wrongly suggest that the
sentence can be necessary without being analytic are the fact that
we normally turn in the vertical axis and the fact that we are not
symmetrical above and below the waist. This conclusion could be
cast in the following form. Suppose that we turned either in the
vertical or in the horizontal axis ; suppose too, that our left and
right sides were symmetrical and also that we were symmetrical
above and below the waist : then, whenever we got into our portraits,
either left and right would be reversed, but not top and bottom,
or else top and bottom would be reversed, but not left and right :
and which of these two reversals would be said to have taken place
would depend either solely on our definitions of ' left ' and ' right ' and
' top ' and ' bottom ' (analytic interpretation of the sentence about
the reflection of my face) or both on our definitions of ' left ' and
' right ' and ' top ' and ' bottom ' and on our movements (synthetic
interpretation of the sentence about the reflection of my face).
And, when it is cast in this form the conclusion suggests the further
question " Could we get into our portraits in such a way that neither
left and right nor top and bottom would be reversed " ?

We could. When we went round behind the mirror instead of
facing it we should merely have to stand with our backs to it.
Then neither top and bottom nor left and right would be reversed,
but back and front would be. So, for this manoeuvre to be successful
we should have to be symmetrical back and front. Finally could
we get into our portraits in such a way that neither left and right
nor top and bottom nor back and front would be reversed ? The
answer to this question seems to be " Not in three-dimensional
space, however symmetrical we were ". And this answer brings me
to the limit which I set myself in this discussion. For assuming that
space is three-dimensional and not inquiring into the nature of this
assumption, I wished to reveal the additional sources of the necessary

truth of the sentence about the reflection of my face. And these additional sources are the definitions of ' left ' and ' right ', which appear to be made superfluous by certain contingent facts. And this appearance is a remarkably clear instance of a fairly common type of delusion.

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ON *SINN* AS A COMBINATION OF
PHYSICAL PROPERTIES

IN a recent article [1] Dr. Paul D. Wienpahl proposes an explication
for Frege's notion of sense that, he believes, " fits the data of Frege's
discussion and does not make sense a subsistent entity " (p. 483).
Wienpahl's proposal is that " the *sense of a sign is the combination of
its physical properties* " (p. 488). But in the face of the requirements
which he has set himself, there seem to be three considerations which
lead to the conclusion that his proposal is defective.

I

Wienpahl's attribution of physical properties to linguistic signs
indicates that he is taking expressions of the kind ' $a = a$ ' and ' a ' to
be physical tokens. Now, although Frege undoubtedly had some
conception of a symbol-token dichotomy, it has never seemed wholly
obvious to the writer whether the entities to which he attributed
sense are to be taken to be tokens or to be symbols. I think a strong
case can be made to substantiate the hypothesis that the latter is
his intention. However, this point need not be debated here, for
Wienpahl's proposal seems to fail on grounds independent of fitting
" the data of Frege's discussion " on this particular issue.

Since symbols (*i.e.* classes of similar tokens) are not physical
things, Wienpahl's assertion, that the sense of a sign is the com-
bination of its physical properties, shows *prima facie* that the entities
to which *he* believes sense may be ascribed are tokens. But one of
the conditions of Frege's discussion which Wienpahl explicitly
attempts to meet [2] is that in expressions (for Wienpahl, inscriptions)
like

$$a = a$$

the sign to the left of the identity sign has the same sense as the sign
to the right of the identity sign. Actually, the condition is the
stronger one that every occurrence of a token of a given symbol has
the same sense as every other occurrence of a token of that symbol.
Clearly, however, no two tokens have the same physical properties
and hence if the sense of a token is the combination of its physical
properties, no two tokens have the same sense. In particular, in
inscriptions like

$$a = a$$

the token on the right may not be said by Wienpahl to have the same
sense as the token on the left.

[1] " Frege's *Sinn und Bedeutung* ", MIND, vol. lix, No. 236 (1950),
pp. 483-494.   [2] *Vide ibid.*, p. 490-91.

II

If Wienpahl has not meant to ascribe senses to tokens but rather to symbols, his proposal is still not tenable for reasons already patent in the foregoing remarks. Since *classes* of tokens do not have physical properties, the proposal thus interpreted would amount to the denial that a linguistic sign has a sense ; and this is surely contrary to the data of Frege's discussion ; and really not seriously to be entertained as Wienpahl's intention either.

III

There is, however, a third possibility. Although classes do not have physical properties, they do have certain *physical-like* properties. Among these, for example, is the property of order in, *e.g.* a class of ordered classes. We can speak cogently of 1 as preceding 2 in the series of positive integers and of the first symbol as preceding in order the second in the class of ordered classes which might constitute the (symbol) statement ' A rose is a rose '.[1] It may be the case then that Wienpahl intended to identify senses not with the combination of physical properties of a token, but with the combination of *physical-like* properties of a symbol. If this is the case, however, one becomes even less sure that Wienpahl has met the second requirement he has set himself ; namely that of dispensing with subsistent entities.

But even if we were to concede that the proposal thus liberally interpreted does no violence to the second requirement, it yet turns out to be unacceptable. Since among the *physical-like* properties of the first symbol in the series of symbols constituted by the symbol-statement ' $a = a$ ' is the property of being first in occurrence in that series, the combination of *physical-like* properties of the first of the symbols will not be the same as the combination of properties of the last of the symbols and hence, according to the proposal, they will not have the same sense.

Although this third possibility is an interesting one, for the reasons mentioned under I, it is unlikely that it is the one intended by Wienpahl. And it seems to the present writer that Wienpahl's difficulties, insofar as he is interpreted as dealing with physical properties of physical inscriptions, stem from his attempt to meet a condition of Frege's which is simply too strong ; namely that in expressions like

$$a = a$$

the two ' a ''s have the *same* meaning.

What is the *raison d'etre* for such a demand ? If the fact that they denote the same entity is brought forward as justification for demanding sameness of meaning, then clearly the argument is

[1] *Cf.* my " A Note on Likeness of Meaning ", *Analysis*, vol. 10, No. 5 (1950), p. 117.

infirm; for so do the expressions 'the morning star' and 'the evening star'. On the other hand, if the argument is based on the assumption of the identity of the expressions, such an assumption is obviously false insofar as the expressions are regarded as physical inscriptions and in any event, quite begs the question.[1] And why should a meeting of the demand be attempted at all? Surely any of the syndrome of problems in discourse which might be handled by the assumption that two expressions have the *same* meaning could be handled with perhaps greater precision by the much weaker assumption that the expressions have a certain degree of similarity of meaning.[2]

Aside from the difficulties which arise out of this attempt to meet Frege's too severe demand, Wienpahl's analysis seems successfully as does Goodman's [3] to dispense with the dubious subsistent entities he seeks to eliminate. Moreover, he has further illuminated the entire problem by arguing effectively for the conclusion (the complement of Goodman's thesis), that there must be difference in signs where there is difference in meaning.

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[1] Currently being discussed in *Analysis*, vol. x, nos. 1 and 5, and vol. xi, nos. 2 and 3, by N. Goodman, C. D. Rollins, R. Rudner, and A. Smullyan.

[2] See, *e.g.* Nelson Goodman's "On Likeness of Meaning", *Analysis*, vol. x, no. 1, pp. 1-7.   [3] *Ibid.*

HETEROLOGICAL AND HOMOLOGICAL

SENECA amuses Lucilius with a confusion between verbal sign and meaning : ' mouse ' is a syllable ; a mouse eats cheese ; therefore a syllable eats cheese. This obvious confusion has extended more subtly into discussions of heterologicality and homologicality.

A word consists of a verbal sign and a meaning. In the word *tiger,* for instance, the verbal sign ' tiger ', an arrangement of five constituent letters, means a large Asiatic striped feline quadruped—using the convenient dictionary definition. Though logical predication uses words (or phrases), only the meanings are the actual predicates. Two perfectly synonymous words, such as *gorse* and *furze* may perhaps be, connect precisely the same meaning with different verbal signs. Though perfect synonyms seldom, if ever, occur, two words can be expositionally supposed to be both perfectly synonymous and predicable. It seems clear that two such words, with their different verbal signs, would be identical predicates because they have exactly the same meaning. The two verbal signs, ' wise ' and ' foolish ', could, though very inconveniently, be interchanged. Since ' Socrates is foolish ' would then mean exactly what ' Socrates is wise ' now means, the predicate is the meaning, and only the meaning. Thus any word used as a predicate may or may not be predicable of its own verbal sign, since its meaning may or may not apply to it. This is also true of phrases.

Predicates are said to be homological when predicable of themselves, and heterological when they are not. According to Bertrand Russell [1] ' " long " is heterological because it is not a long word, but " short " is homological '. He evidently calls the word *long* " not a long word " because its verbal sign ' long ' is not lengthy, and calls the word heterological because its meaning does not apply to its verbal sign. Consistently with this he calls the word *short* homological, for its meaning, little in length, does apply to its verbal sign. If the meanings of the two words are the actual predicates, the word *long* is not necessarily impredicable of itself simply because it is not predicable of its own verbal sign. Neither is the word *short,* simply because its meaning is predicable of its own verbal sign, homologically predicable of itself.

If the verbal sign ' red ', for instance, is printed in red ink, the meaning, or predicate, necessarily applies to it. In the word *written* the meaning obviously applies to the verbal sign ' written ' if this is in handwriting. Such verbal signs are temptations to call their meanings, their predicates, homological. If the verbal sign ' red ' is in black, or the verbal sign ' written ' is in print, it is

[1] *An Inquiry into Meaning and Truth,* 1940, p. 79.

a temptation to call the word heterological because the meaning does not apply to the verbal sign. Such varying homologicality or heterologicality seems to be casual and spurious since the word *red*, or *written*, has the same meaning, and is thus the same predicate, however the verbal sign is legitimately varied.

Russell calls the word *German* heterological, and the word *English* homological. This seems to be an anomalous distinction between two such parallel predicates. The meaning of *German* cannot be predicated of its verbal sign ' German ' since this is not part of the German language. In the word *English* the verbal sign ' English ' is part of the English language, and the meaning does apply to it. Thus Russell apparently calls *German* heterological, and *English* homological, because the meaning does not apply to the verbal sign in the former, and does so apply in the latter. This anomalous distinction between two such comparable predicates seems to be incompatible with genuine heterologicality and homologicality.

Seneca used the confusion between verbal sign and meaning playfully ; Russell, by failing to identify the predicate with the meaning alone, seems to confuse a logical issue.

Ryle [1] calls ' written ' in manuscript, in writing, a self-epithet. The meaning of the word *written* obviously applies to the hand-written verbal sign. He calls ' written ' in print a non-self-epithet. The meaning of the word *written*, in handwriting, obviously does not apply to the printed verbal sign. ' Orthographic ', Ryle adds, is a self-epithet " only *because* ' orthographic ' both has and stands for a certain philological property, namely that of being correctly spelled ". Since the verbal sign is correctly spelled, the meaning of the word *orthographic* is obviously predicable of the verbal sign ' orthographic '. The applicability of the predicate, being correctly spelled, to its own verbal sign does not affect its predicability or non-predicability of itself. When Ryle calls ' obscene ' a non-self-epithet it would be futile to consult the verbal sign to determine whether the meaning of the word *obscene* is predicable of the meaning itself, though it seems fairly evident that neither the verbal sign nor the meaning nor the two together is itself obscene.

In the word *polysyllabic* the meaning obviously applies to the verbal sign. In the word *English* the meaning, or predicate (say), the language used in Britain, is not so directly seen to apply to the verbal sign by merely inspecting it. Since the verbal sign ' Englisch ' or ' Anglais ' has the same meaning as ' English ', the language used in Britain must be known to contain the verbal sign ' English ' before the meaning of the word *English* is known to apply to that particular verbal sign. Ryle calls ' English ' a self-epithet. Apparently, in the course of his analysis, he applies the term self-epithet where in actual fact the meaning of the word does apply to its own verbal sign. This seems to be so in his instances. Calling ' English ' a self-epithet, he affirms, does not affirm that it is an

English word. This is so because the application of the meaning to the verbal sign is not evident by inspection of the sign itself, as it is in ' polysyllable '. In the case of ' French ', which he calls a non-self-epithet, the predicate or meaning (say), the language used in France, does not apply to the verbal sign, for this belongs to the English language. In all instances, apparently, where Ryle applies the term non-self-epithet the meaning of the word does not apply to the verbal sign.

The relation within a word between meaning and verbal sign has disturbed logical analysis. In many instances at least predicates have been called homological when the meaning applies to the verbal sign, and heterological when it does not. Ryle has reached the distinction between self-epithet and non-self-epithet by his own route, but the former, in actual fact, seems to be used when the meaning does apply to the verbal sign, and the latter when this is not true. The correlation is clear during the change-over when the verbal sign ' written ' alters from handwriting to print. In hand-writing ' written ' is a self-epithet, and the meaning applies to it ; in print, when the meaning does not apply to it, it becomes a non-self-epithet.

Though a predicate is not homological *because* it applies to its verbal sign, it may actually be so, as it may be heterological, though not because it is not so applicable. Ryle apparently includes ' obsolete ' among the non-self-epithets. The meaning of the word *obsolete*, out of use, does not apply to the verbal sign ' obsolete ', and also, since it does not apply to itself, it seems to be genuinely heterological.

Heterologicality presents no difficulty, for a predicate is essentially predicable of something else. In the word *polysyllabic* the meaning obviously applies to the verbal sign ' polysyllabic ', but a poly-syllabic meaning or predicate seems odd. Whenever the verbal sign directly invites the predicate the same oddness seems to appear. The meaning of the word *short* seems to be curiously said to be itself short however clearly it applies to the verbal sign. Red seems to be oddly predicated of the meaning, though it is appro-priately applied to the verbal sign when this happens to be red.

Even when the verbal sign does not directly invite its meaning to be applied to it the oddness seems to remain. The meaning of the word *English* is curiously said to be English, and suggests a confusion of meaning with the meant. Apparent oddity is, of course, no logical guarantee. It is, however, at least possible that the applicability of some meanings to their verbal signs has promoted the myth of the homological predicate predicable of itself. If this type of predicate is actually mythical, the word *heterological* is superfluous, since all predicates are then impredicable of them-selves, and the word *homological* is delusory.

The meaning of the phrase *not ferocious*, however, applies to its verbal sign, and, if a predicate ever can be self-predicable,

also to itself. Many negative predicates, Bertrand Russell [1] notes, are predicable of themselves. These are usually also applicable to their verbal signs—the meanings of the phrases *not sinister* and *not raucous* for example. If the meaning, or predicate, can apply to both its verbal sign and itself, the homological predicate is not a myth. Perhaps a homological predicate, genuinely predicable of itself, always does apply to its verbal sign, though it can do so without being actually homological. The meaning of the phrase *not predicable of itself*, the actual predicate, seems to apply to the verbal sign, for this is not predicable of anything, but this does not decide whether the heterological predicate is also homological. The relation between meaning and verbal sign within the word should, however, be kept carefully in mind, if only because some predicates have been erroneously called homological because the meaning applies to the verbal sign.

JOSHUA C. GREGORY.

[1] *The Principles of Mathematics*, 1937, p. 80.

LOCKE AND THE FIRST EARL OF SHAFTESBURY :

ANOTHER EARLY WRITING ON THE UNDERSTANDING

THERE is in the Public Record Office an unprepossessing and imperfect manuscript which may well be a version of Locke's first thoughts on the Understanding slightly earlier than anything previously noticed. It was preserved by the family of the famous Restoration statesman, Anthony Ashley Cooper, 1st Baron Ashley and after 1672 1st Earl of Shaftesbury.[1] Elsewhere in Shaftesbury's papers, moreover, there is evidence that he possessed in 1681 a manuscript which looks remarkably like an early form of Locke's Essay, perhaps this document itself. " An Essay Concerning Humane Understanding " was not actually printed at its full length, and in its final form, until 1690.

This evidence undoubtedly complicates the already intricate problem of how the Essay had its origin, and how it developed up to the point of its publication. Two other manuscripts are known, both dated as early as 1671, which can fairly claim to be drafts of the work, and the fact that yet a fourth document of a similar nature, dating from about 1685, once existed must also be taken into account, although its present whereabouts is unknown. The two 1671 drafts were preserved by Locke's heirs, the family of King, later Barons King and still later Earls of Lovelace, and they were noticed as early as the year 1829 when the 7th Lord King wrote his *Life of Locke*.[2] The later of these Lovelace drafts, now known as Draft B, was printed by Professor Rand in 1931, but without any notice of its earlier form (Draft A), which was subsequently edited with great care by Professor Aaron and Mr. Gibb.[3]

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[1] All the Shaftesbury papers were presented to the Public Record Office in 1871—see the " Report to the Deputy Keeper of the Public Records on the Shaftesbury Papers ", by W. Noel Sainsbury, 1871. This document is in the bundle of Locke papers, present ref. PRO/30/24/47. The Shaftesbury papers do not seem to have been much used by students of Locke since Fox Bourne quarried them for his standard " Life ", 1876, though most scholars refer to Sainsbury's report.

[2] See p. 6. The Lovelace Papers are now in the Bodleian Library (see W. Von Leyden in " Sophia " (Padua), Anno XVII, 1949), but the Earl of Lovelace retains Draft A, together with a magnificent library of Locke's own books.

[3] Draft B was published by Harvard University Press. " An Early Draft of Locke's Essay ", edited by R. I. Aaron and Jocelyn Gibb, was published by the Oxford University Press in 1936. It is to their admirable introduction, and to Aaron's " Locke ", 1937 (see pp. 54-59), that we owe most of our knowledge of how the Essay was written, and the terms Draft A and B.

But it should not be a matter for surprise that another, and even more inchoate, form of the work should be found in the Shaftesbury Papers, so far away from the others. My own researches into Locke's political career and into the origins of his *Two Treatises of Government* [1] make it clear that his position in Shaftesbury's household after 1667 was such that much of his intellectual activity must have gone forward in Shaftesbury's company and have been directed towards Shaftesbury himself. Locke undoubtedly deeply respected not only Shaftesbury's political opinions and conduct, but also his political theory, his attitude to Toleration—perhaps to the theory of knowledge itself. The true nature of his intellectual relationship with Shaftesbury remains to be described.

The Shaftesbury manuscript can be given a first classification as a variant version of Draft A, and can be conveniently referred to as A(i). It is certainly very closely connected with that earliest of the other drafts, and the provisional conclusion of research must be that it is in fact, in part or in whole, a few days earlier than Draft A, and so the first of Locke's writings on the Understanding known to be extant. The presentation of detailed evidence on this fine point of priority between A(i) and A would be complicated, technical and of interest more, perhaps, to the manuscript historian than to the philosopher : the object of the present note is primarily to call attention to the existence of A(i). But there are one or two straightforward points, and the first of them is that the manuscript dates itself ten days earlier than A. In the famous passage in Draft A (p. 41) where Locke says, " For having seen water yesterday it will always be an unquestionable true proposition to me that water did exist 10° Jul.71 ", in A(i) he says " 1° Jul'71 ". And where in A he gives a list of nine ideas, and then goes on to refer to the list as if it contained only eight (Draft A, p. 27), A(i) actually has only eight listed. These would seem to be conclusive by themselves, though the possibility of a mistake in transcription must not be overlooked in the case of the date, but they are reinforced by more general considerations. Draft A in its printed form is the result of extensive rearrangement carefully carried out by its editors in accordance with Locke's directions on his own MS., but A(i) has no such directions, and is in the earlier order entirely. Moreover, there would seem to be only two possibilities for A(i) : it is either an earlier version of A or a revision of it. Now whatever A(i) is, it is not a revision—it is incomplete in every sense : it is a rough working, written not by Locke, but by a rather ignorant amanuensis, probably at Locke's dictation.[2] But though there is such clear evidence for

[1] To be published in the introduction of a critical edition of that book by the Cambridge University Press. This is only one instance of continuity between the Shaftesbury Papers and the Lovelace Collection.

[2] This is to be inferred from the numerous blanks left for future filling, and from the occurrence of a word like " dicatoris " for " de caeteris ", etc. But the MS. is endorsed unmistakeably by Locke himself, " Intellectus ".

placing A(i) just before A, there are also serious difficulties, including the presence in the later part of A(i) of page references which can only intend the pagination of A.[1] The problem can only be resolved, it would seem, by supposing that Draft A was begun before A(i) was finished, and that, at least between 1st and 10th July, 1671, Locke was both dictating in the rough on to the papers now in the Record Office and also himself copying what he had dictated earlier into the Commonplace book in the Lovelace Collection. The two versions, therefore, can be regarded as practically simultaneous, but with interesting variations in detail, variations which would take us beyond the present purpose. It gives us a fascinating glimpse of a great philosopher at work.

But not all of the Shaftesbury Manuscript consists in a variant version of Draft A. There is a page and a half of it which, though it comes in the middle of the Draft as it is now written, was evidently on its sheet before that Draft was begun, and indeed was ruled off by the writer so as to keep it out of his text. It consists in the main of three successive versions of a passage which appears as para. 6 of Draft A, together with half a page of material which is not to be found anywhere else. This fragment is obviously nearer to those " hasty and undigested thoughts "[2] with which Locke tells us his whole project in fact began, than anything else which has survived, and may actually be a part of them.

On 1st July, 1681, the Privy Council issued a warrant for the arrest of the Earl of Shaftesbury, an information of treason having been made against him. A second warrant directed Francis Gwyn, one of their Clerks, " to make diligent search for the papers of the Earl of Shaftesbury, and to seize and seal them up and bring them so sealed up before the Privy Council at Whitehall at 9 a.m. to-morrow ". Five lists of the papers which Gwyn bore off to the Council survive in the PRO, and one of them has the following heading :[3] " The Papers wch. were in my Lds. Closett in ye gallery seizd by Mr. Gwyn and brought back from ye Councill by Wilson. July 6° 1681 ". On this list are fourteen titles of documents, and a miscellaneous lot they were, as can be seen from the following entries :—

> " The Horse book
>
> A few memdms of yor Ldps abt fatning Chickens
>
> Gardning etc

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[1] *E.g.* the note (p. 47 of Draft A) specifying pp. 81, 82, 83, which can only be pages of the Commonplace Book in which Draft A was written.

[2] See the " Epistle to the Reader " of the published Essay, and Aaron's discussion of its meaning.

[3] See Cal. S. P. Dom., Ch. II, 1680-81, p. 339, and Brown, L.F., *The 1st Earl of Shaftesbury*, N.Y., 1933, p. 284. The lists are in PRO 30/24/VIA, No. 349.

A large pap^r intituled y^e Intellec^l Pow^r.-

4 sheets beg$\overline{g}$: / The Light of Nat^e is reason sett up
in ye soule att 1st by God. in mans - Creac$\overline{on}$: 2nd
by Christ

Mr Hughes draught of ye Garden^s."

It is hard to resist the inference that the third of these titles covers
some form or other of Locke's Essay in its early state. The most
economical hypothesis is that it was Draft A(i), for the title and
beginning of that document are missing, and it might well have
been best described as " A large pap^r intituled ye Intellec^l Pow^r",
the adjective " large " being the natural way of referring to the
folio sheets on which it is written. This description seems rather
unlikely to have covered Draft A or B, for the title of neither fits it
well, though the paper could have been an entirely different draft,
or something more than a draft since exactly ten years had gone by
since A, A(i) and B had been written. The missing fourth early
version of the Essay, which seems to have been overlooked in
accounts of its development, could have existed in July, 1681,[1]
but it is difficult to suppose that it is in question here. But what-
ever this document was, this must surely be the only occasion on
which two such manuscripts,[2] one of them perhaps an early form of
a great philosophical classic, have been seized in the hope of making
good a charge of treason against a famous statesman.

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[1] It is described by its early nineteenth century owner as " A MS copy
of Mr. Locke's Essay . . . , in a small 12mo book, . . . full of corrections
in the author's own handwriting. There is a date 1685 to this book but . . .
it is evidently inserted at a subsequent period ". See *Original Letters of
John Locke, Alg. Sidney etc*, edited by T. Forster, 1830, 2nd. ed. 1847. The
fate of most of Forster's collection is as yet unknown.

[2] The fourth entry in the list above is almost as intriguing as the third.
Mr. Von Leyden suggests that it cannot have been anything of Locke's,
and that these two lines are the opening of an epistemological essay by the
Presbyterian Shaftesbury himself.

VI.—CRITICAL NOTICES

An Examination of the Place of Reason in Ethics. By S. E. TOULMIN.
  Cambridge : University Press. Pp. xiv + 228.

IT is easy to state Mr. Toulmin's main problem and to formulate in
general terms his attempted solution of it, but when one comes to
details there are things in his book which I find obscure and puzzling.
I will therefore begin with what is clear to me, and will then raise
some questions about the parts which I do not fully understand.

The main problem may be put as follows. In the course of our
private reflections and our discussions with others we seem *prima
facie* to be constantly offering factual propositions as *reasons* for
or against moral conclusions. Moreover, we constantly talk as if
some of these reasons were *valid* and others were *invalid* for the
purpose for which they are adduced. Thus, *e.g.*, a person might
assert that he is under an obligation to pay a certain sum of money
to another to-morrow, on the ground that he has borrowed that sum
from him and that to-morrow is the agreed date for repayment.
Another person might allege that he is under an obligation to challenge
a certain man to a duel, because the latter has publicly insulted him.
Most civilized men would consider the former to be a valid reason
and most contemporary Englishmen would consider the latter to be
an invalid one. Mr. Toulmin argues that such *prima facie* appear-
ances must be accepted at their face-value, and his main problem
is to determine what kind of factual premisses are valid reasons for
moral conclusions.

In Chapter X Mr. Toulmin considers what *kind* of reasons (whether
valid or invalid in particular cases) are regarded as *relevant* to speci-
fically moral questions. He develops his answer further in Chapter
XI. His conclusions may be summarized as follows. (1) There are
three main cases to be considered. (i) Where what is under dis-
cussion is what ought to be done on a particular occasion, and where
one of the alternatives is unambiguously in accord with a maxim
commonly accepted in the community to which the persons discussing
the question belong, whilst the other alternatives unambiguously
conflict with one or another of these maxims. (ii) Where what is
under discussion is again what ought to be done on a particular
occasion, but where each of the alternatives seems to conform
to some and to conflict with others of the maxims in question.
This is the case of a ' conflict of duties '. (iii) Where what is under
discussion is, not the rightness or wrongness of a particular action,
but that of some rule in a particular code of morals or even of that
code as a whole. (2) In the first case, and only in it, is it considered
a sufficient reason to refer to the accepted maxim in question, and
to argue that a certain alternative is the only one which would accord

with it. In the other two cases it is considered a sufficient reason
to appeal to the principle that 'preventable suffering is to be
avoided'. These are the only two types of reason which Mr. Toulmin
finds to be generally admitted as relevant in specifically moral
arguments. (3) It will be noted that the second type of reason is
stated in a *negative* form, *viz.*, the prevention of avoidable suffering
and frustration. Mr. Toulmin adds, however, that this is only a
minimal requirement. We can, and often do, ask whether, if the
accepted code were changed in certain directions, the members of
the community would 'lead fuller and happier lives '. If there are
good reasons for believing this, that would be a sound argument
for making the change, even though no positive hardship were in-
volved in the existing code. Nevertheless, he says, the *primary*
application of the notions of ' ought ', ' right ', ' duty ', ' morality ',
etc., is in reference to ' actions or institutions which may lead to
avoidable misery for others ' or even for oneself. It is an extension
(though a very natural one) of these notions to use them where the
question concerns the chance of deeper happiness for others or even
for oneself. Mr. Toulmin describes the principle that preventable
suffering should be avoided as the ' overall principle ', and says that
this cannot be rejected without abandoning the very idea of ' duty '
and of ' morality '.

It would appear from the above that Mr. Toulmin's answer to his
question is a form of a very old and highly respectable ethical theory,
viz., hedonistic utilitarianism. There is little, if anything, in it
with which Sidgwick, *e.g.*, would have quarrelled, though Sidgwick
argues his case with enormously greater detail and sublety, and com-
bines it with the acceptance of certain very abstract principles of
distribution which he regards as self-evident and does not attempt to
base on utilitarian considerations. Notwithstanding the passages
which I have quoted, which seem to support this interpretation,
there are others which seem to suggest a different view. Thus, in
Chapter X Mr. Toulmin says that morality never becomes *wholly*
teleological, and states as his reason that the code current in a given
society remains obligatory on its members in all cases in regard to
which it is unambiguous.

He reverts to this point in Chapter XI. So far, he says, he has
been considering only the question : How do we *in fact* profess to
distinguish valid from invalid reasoning in moral questions ? He
now raises the question : What *makes* some such reasoning valid and
some invalid ?. I take it that this should mean : Can one explain
satisfactorily why the arguments which pass these tests are valid
and why those which fail to do so are invalid ? From what Mr.
Toulmin says in other parts of his book about similar questions which
have been raised about the types of reasoning commonly accepted
as valid by scientists in scientific reasoning I conclude that his
answer would be that such a question is improper and meaningless.
But what he actually discusses here is whether we can reasonably

ask for *a single* test for the validity of moral reasoning, which shall apply to all the three cases which he has distinguished. This is obviously a different question. Nor does he discuss even this directly. Instead he asks whether either of the two tests which he has mentioned could be dispensed with in favour of the other. This again would seem to be a different question.

Mr. Toulmin's answer to it is as follows. If we adopt conformity to a current moral code alone as our test, we shall never be able to question the rightness of any such code. In that case ' morality ' will collapse into mere ' authority '. If, on the other hand, we adopt the utilitarian test alone, ' morality ' will collapse into mere ' expediency '. Now we use the words ' moral ' and ' morality ' in such a way that neither arguments from mere authority nor arguments from mere expediency count as *moral*.

I should have thought that the utilitarian, at any rate, had an easy answer to this, and that Mr. Toulmin had in effect given it in other parts of his book which I have already quoted. The utilitarian would agree that, as a matter of fact, the feeling of obligation is *directly* attached to acts which accord with the moral code accepted in one's community, and that it is not in general mediated by any thought of utility. He could argue very plausibly that it is of the utmost utility that this should be so. But he would say that the only ultimately *valid* reason for the principles of any moral code is their general utility ; and that the only *valid* reason for obeying them in particular cases where disobedience would have greater *first-order* utility than obedience is the various kinds of *second-order* disutility which arises from any breach of a generally accepted and generally useful set of rules.

Now, there are certain well-known theories as to the correct analysis of what I will call ' moral indicatives ' which would seem to allow no place for reasoning in moral topics. Mr. Toulmin discusses the two main types of such theories under the titles of ' the Subjective Approach ' and ' the Imperative Approach '.

The subjectivist allows that moral indicatives do, as their grammatical form suggests, express *judgments* about actions, intentions, motives, etc. But he alleges that what a person who makes such a judgment is asserting in any case is simply that the action in question evokes in him, or in some class of persons of which he is a member, a certain kind of emotional reaction towards it. It is obvious that on this view there can be no question of presenting reasons, valid or invalid, for or against a person's moral judgments. It is alleged by subjectivists that what are called ' arguments ' on moral questions are really certain psychological techniques for altering men's emotional reactions. Since no question of truth or falsity can enter here, the subjectivist denies that we can talk of ' validity ' or ' invalidity ' in any literal sense. What a person calls a ' valid ' type of moral argument is simply a technique for altering emotional attitudes, which happens to appeal to him.

I do not know that there is any way of answering such contentions except by pointing out that they are not in accord with common-sense and common usage and by asserting that the latter have a *prima facie* claim to acceptance. This, at any rate, is all the answer that Mr. Toulmin gives. His counter-assertions amount to the following. (1) We *do* in fact distinguish moral *arguments*, valid and invalid, from admonitions, threats, rhetorical propaganda, and so on. (2) We *do* in fact distinguish moral arguments as ' valid ' and ' in-valid ', and we mean by these terms ' *worthy* of acceptance ' and ' *unworthy* of acceptance ', respectively. By calling such an argu-ment ' *worthy* of acceptance ' we do *not* just mean that it is in fact effective when addressed to ourselves. (3) Again, in calling some-thing ' morally good ' or ' morally right ' the speaker is *not* just recording the fact that he reacts favourably to it. He is asserting in the one case that it is *worthy* to be approved, and in the other that it is *worthy* to be adopted. (4) To say that something is worthy of approval is to say ' that there really is a valid argument (a good reason) for . . . approving of it and for recommending others to do so too '. (P. 39).

Mr. Toulmin gives the name ' imperativist ' to those who hold that moral indicatives do not express *judgments* of any kind, but do serve to evince certain emotions or desires of the speaker and to evoke certain emotions or stimulate certain actions in his hearers. In discussing this type of theory he confines himself to the doctrine that moral indicatives express, in a grammatically misleading form, what would be more correctly expressed by uttering a sentence in the imperative. Now, as he admits, there is no doubt a perfectly good sense of ' reason ' in which one can ask : ' What was *X*'s reason for giving that order ? ' or ' What reason is there why *Y* should obey that order of *X*'s ? ' But ' reason ' here means *motive* for or against acting in a certain way, whilst Mr. Toulmin is concerned with ' reason ' in the sense of *ground* for accepting something as true or rejecting it as false. In that sense it is plainly nonsensical to talk of reasons for or against a command, since the alternatives ' true ' and ' false ' do not apply to it. Now it seems to him to be a plain fact that in moral contexts we do give reasons, in the sense of grounds for accepting as true or rejecting as false, and that we do distinguish between those which are valid and those which are invalid. He concludes therefore that the imperativist analysis of moral indicatives cannot be correct.

There remains a third well-known analysis of moral indicatives, which Mr. Toulmin discusses elaborately and rejects. This is the doctrine that such sentences express judgments to the effect that an action or an experience, etc., has one or another of certain ob-jective *properties*, of which such words as ' morally right ', ' morally good ', etc., are names.

Mr. Toulmin begins by distinguishing three types of property, viz., simple, complex, and scientific qualities. Both simple and complex

qualities are 'directly perceivable'. A simple quality cannot be defined without circularity either in terms of simpler qualities or in terms of any standard set of operations. An example would be yellow, in the sense in which it is mentioned in the sentence : 'That looks yellow to me from here now'. A complex quality can be defined in terms of a certain standard operation, *e.g.*, counting, which must be gone through before it can safely be asserted or denied of a perceived object. An example would be the property of being heptagonal. A scientific quality is one which cannot be perceived *directly* and may not be perceptible at all, but whose presence or absence is detected by a certain standard operation. An example would be yellow, in the sense in which it is mentioned in the sentence : 'The sun is really yellow, though it appears red at sunset'.

Mr. Toulmin asserts that *all* properties fall under one or other of these three headings. He also asserts that this is true too of psychological properties, such as 'haughty' and 'meek'. (I do not see how this can possibly be so, unless 'perception' is extended to cover introspection.) He concludes that anyone who alleges that goodness, *e.g.*, is a property of the entities which are called 'good', *must* be asserting that it is a quality which falls into one or other of these three classes.

He then tries to show that goodness, rightness, etc., cannot be *directly perceived* qualities, whether simple or complex. Unless I am mistaken, he does not discuss the alternative that they are 'scientific qualities'. The course of his argument appears to be as follows.

(1) Suppose that P is a directly perceived quality (whether simple or complex), and suppose that A says 'S *is* P' and B says 'S is *not* P' where both are contemplating the same object. Then this difference *could* arise only from one or other of the following sources. In the case of a simple quality it must arise either (i) from deliberate or inadvertent misuse of the word 'P' by one party, or (ii) from the fact that 'P' has different meanings or different limits of application for the two, or (iii) from the presence in one of them of a relevant 'organic defect' such as colour-blindness. In the case of a complex quality the difference must arise either from one of these sources, or (iv) from incorrect application of the standard technique for ascertaining the presence of the quality, or (v) from the use of different and incompatible verbal definitions of the word 'P'. (2) It is *logically* possible that A should say : 'S is *good*' and B should say : 'S is *not good*' where both are contemplating the same object, even though *none* of these sources of difference existed. No doubt it might *in fact* be the case that any pair of persons would agree in their moral judgments under such conditions ; but, if so, this would be purely contingent, like the fact that practically everyone dislikes the sensation which arises if you stick a pin into his body. (3) We may therefore conclude that words like 'good' and 'right' are not names of directly perceived properties.

What are we to say of this argument ?   (1) It is of the same verbal
form as the following argument.   Every regular solid *must* be either
a tetrahedron or a cube or an octahedron or a dodecahedron or an
icosahedron.   *This* has neither of these shapes.   Therefore *this*
cannot be a regular solid.   But there is an important difference.
The premiss in the above argument is a necessary consequence of the
agreed definition of ' regular solid ' and the axioms of Euclid ;
it is not a mere assertion about the sense in which the word ' regular
solid ' is commonly used.   The corresponding premiss in Mr.
Toulmin's argument is admittedly a mere assertion as to the way
in which the word ' property ' is commonly used.   In such a case one
is inclined to suspect that the exhaustive set of alternatives has been
chosen by the author in such a way as to exclude the proposition
which he wishes to reject.   It is rejection by verbal legislation, and
it is unlikely to appeal to any but the already converted.   (2) If
the phrase ' organic defect ' in one of Mr. Toulmin's alternative
sources of difference is taken literally, it is irrelevant to the case of
' good ' or ' right ' ; for no-one supposes that these are names of
qualities which are literally perceptible by any of our *senses* or by
any conceivable extension of them.   But, if it is interpreted more
widely, the objectivist would be likely to say that, when all other
sources of difference between *A* and *B* have been eliminated, the
conflict between their moral judgments must arise from a moral cog-
nitive defect *analogous to* colour-blindness, which he would describe
as ' moral blindness ' or ' moral insensitiveness '.

It is plain that Mr. Toulmin has the second of these contentions in
mind in the argument which he uses on pp. 23 to 25.   He imagines
the case of a person who habitually exhibits good moral qualities,
performs right actions, and gives what are admittedly valid moral
reasons for doing them.   He imagines such a person being asked
whether, when considering what he ought to do, he is aware of any
' non-natural property of fittingness ' in the alternative which he
decides to enact.   The person is supposed to answer that he does not,
that he decides to enact a certain alternative because there is a
valid moral reason for doing so, and that he is not interested in any
additional ' non-natural property ' of the alternative.   Mr. Toulmin
asserts that the objectivist would have to say of such a man that
he may know what things are good but cannot know what goodness
is, and that he is like a colour-blind man who has learned a technique
for distinguishing red things from green ones but is missing an
essential experience.   This, Mr. Toulmin thinks, would be ridiculous.
The reason that he gives is that we use the phrase ' to know what
goodness is ' in such a way that it is equivalent to ' being virtuous
and upright and giving good reasons for one's actions '.

I should doubt whether this argument will produce much impression
on those to whom it is addressed.   In the first place, they might
legitimately object that the supposed virtuous person is a puppet
of Mr. Toulmin's creation, whose answer is put into his mouth by his

maker. We may safely assume that no Gallup poll has ever been taken of the answers of virtuous persons to Mr. Toulmin's question. Secondly, it might be objected that a plain man, however virtuous, could hardly be expected to return any very reliable answer to a question put to him in technical philosophical terminology (' nonnatural properties ') which is unfamiliar to him.

Mr. Toulmin alleges, not only that the objectivist account of moral indicatives is false, but also that it prevents those who hold it from paying adequate attention to the question which most interests him, *viz.*, the question of valid and invalid reasons for moral judgments. This is an historical statement against which, I should have thought, there is plenty of evidence. I suppose that Sidgwick and Moore and Ross may fairly be cited as distinguished exponents of the objectivist doctrine. None of them has neglected to discuss seriously the questions : ' What makes right acts right ? ' and ' What makes good things good ? ' But surely this is the form which Mr. Toulmin's question about valid and invalid reasons in morals takes, when formulated in terms of an objectivist view of the nature of moral indicatives.

Much more valuable than Mr. Toulmin's detailed arguments against the subjectivist, the imperativist, and the objectivist accounts of moral indicatives is his own estimate of the strong and the weak points in each of them and of the places of each in a correct and adequate account of moral phenomena. Leaving aside for the moment certain features which I find obscure. I think that his main points may fairly be summarized as follows.

(1) Moral phenomena in general, and the experiences which we express by moral indicatives in particular, are unique and peculiar. The only satisfactory way to investigate them is to do so *directly*. If we try to force them into moulds derived from reflecting on nonmoral phenomena and the verbal expressions for these, we shall inevitably distort them. (2) The experiences which are expressed by moral indicatives resemble in certain respects judgments assigning an objective property to a thing, in certain respects judgments asserting an emotional reaction of the speaker towards an object, and in certain respects the experiences which are naturally expressed by uttering interjections or sentences in the imperative. But in each case there are unlikenesses which are as important as the likenesses. (3) Each of the three theories has arisen through concentrating on the resemblance to one of these non-moral parallels and ignoring the unlikenesses to it and the resemblances to the other non-moral parallels. (4) The two theories which admit that moral indicatives express *judgments, viz.*, the objectivist and the subjectivist theories, agree in making a certain tacit assumption. They both assume that two judgments about the same thing can logically conflict only if they refer to one and the same *property*, which one person assigns to the object and the other denies of it. Seeing that moral judgments can logically conflict, the objectivist concludes that the words

'good', 'right', etc., must be names for properties of a peculiar
kind. Seeing that there are no such properties, the subjectivist
concludes that moral judgments cannot logically conflict and there-
fore can only assert or deny that the speaker is reacting emotionally
in a certain way to the object.   (5) Mr. Toulmin rejects this common
assumption.   He holds that moral judgments *can* logically conflict,
but that they do *not* assert or deny a property of an object.   In order
that they may logically conflict " all that is needed is a good reason
for choosing one thing rather than the other.   Given that, the
incompatibility of ' This is good ' and ' This is not good ' is preserved.
And that, in practice, is all that we ever demand " (p. 43).   (6) Mr.
Toulmin thinks that the imperativist makes the same tacit assump-
tion as the objectivist and the subjectivist.   But the imperativist's
reaction is to deny both their alternatives and so to conclude that
moral indicatives do not express judgments of any kind.   (7) This
line of thought is made plausible by concentrating upon singular
sentences describing a concrete perceptible fact, like ' The cat is on the
mat ', and taking them as the type of all sentences which can possibly
express judgments.   Here and here only it is sensible to talk of a *cor-
respondence* between the elements and the structure of the sentence,
on the one hand, and those of a certain fact to which the sentence
refers, on the other, and to say that truth or falsity consists in the
concordance of discordance between sentence and fact.   Since
moral indicatives plainly do not answer to this pattern, it is assumed
that they cannot be true or false, *i.e.*, that they cannot express
judgments.   But, then, it must be noted that the vast majority
of sentences which admittedly express judgments obviously do not
fit into this pattern.

I find myself in general agreement with much in Mr. Toulmin's
position as thus summarized.   What I do not clearly understand
is his own positive account of what is expressed by moral indicatives.
One aspect of this is stated in Chapter VI under the heading ' Gerun-
dive Concepts '.   These concepts fall under the general formula
' *worthy* to be treated in a certain way '.   Thus a ' true proposition '
is one which is worthy to be believed, a ' valid argument ' is one which
is worthy to be accepted as making its conclusion worthy to be
believed, a ' beautiful object ' is one which is worthy to be admired
aesthetically, a ' morally good disposition ' is one which is worthy
of moral approval, and a ' morally right alternative ' is one which is
worthy to be chosen for enactment.   Mr. Toulmin states definitely
that gerundive concepts cannot be identified with or defined wholly
in terms of *de facto* subjective attitudes.   To think that they can is
the typical ' naturalistic fallacy '.   There is nothing particularly
new or startling in this aspect of the theory.   It has been very fully
developed by, *e.g.*, Sir W. D. Ross and by Dr. Ewing.   ' Worthiness
to be treated in a certain way ' is in fact our old friend ' fittingness ',
and, as such, I have no quarrel with it.

The other aspect of Mr. Toulmin's theory is the identification of

' x is worthy of approval ' with ' there is a *valid reason* for approving x '; and the repeated assertion that logical conflict as to the goodness of x or the rightness of y reduces to conflict as to the *validity of alleged reasons* for approving x or for doing y as the case may be.

Now this raises the following question. Does ' approving x ' mean ' feeling a certain kind of emotion towards x ' or ' judging that x is a worthy object of a certain kind of emotion ' ? On the first alternative the words ' valid reason ' in the phrase ' valid reason for approving x ' cannot be used in the sense in which they are used in the phrase ' valid reason for accepting so-and-so as true or rejecting it as false '. Yet it seemed to be an essential part of Mr. Toulmin's case against the view that moral ' arguments ' are only a psychological method of altering or confirming men's emotional attitudes that such ' arguments ' present ' reasons ' in the sense of grounds for accepting something as true or rejecting it as false. Suppose, on the other hand, that we take ' approving x ' to mean ' judging that x is a worthy object of a certain kind of emotion '. Then, no doubt, ' reasons ' can be taken to mean ' grounds for accepting as true or rejecting as false '. But in that case I do not see how a conflict between ' x is good ' and ' x is not good ' can possibly reduce to a conflict as to the validity of the reasons alleged for approving x. Surely the conflict will be simply as to whether x is or is not a worthy object of a certain kind of emotion or not. It might be so, even though the alleged reasons for believing it to be so were invalid. And, whether they are valid or not, the question of their validity or invalidity is one thing, and the question whether the conclusion for which they are adduced is true or false is another.

In conclusion I must mention that quite a considerable proportion of Mr. Toulmin's book is taken up with discussions about the nature of reasoning and explanation in natural science, about the distinction between ' appearance ' and ' reality ' there and in ordinary life, and so on. These parts of the book contain much interesting matter, but I must pass them over here. Their relevance is supposed to be this. They support the view that the nature and the criteria of *moral* reasoning must be ascertained by a *direct* study of such reasoning and of the contexts in which it occurs. They do so by pointing out that the same is true *mutatis mutandis* of non-moral reasoning in pure mathematics, in natural science, and in the affairs of daily life.

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Zwei aristotelische Frühschriften über die Ideenlehre. By PAUL
WILPERT. Verlag Josef Habbel, Regensburg, 1949.

THIS book deals with two lost works of Aristotle, the *Περὶ Τἀγαθοῦ*
and the *Περὶ Ἰδεῶν*, both written near the time of Plato's death.[1]
Dr. Wilpert's central objective is to reconstruct and interpret the
contents of these works ; and since he expressly abstains from the
thorough analysis of *all* Platonic and Aristotelian writings which—
as he remarks—must be made before any conclusions as to late
Platonic or early Aristotelian views can be regarded as firmly-
founded his own inferences about these views must be taken as only
tentative or at least only partially established. I shall, however, for
its general interest, first reproduce the general picture he paints
and then consider critically the detailed discussions from which this
picture emerges.

A change in Plato's doctrine took place (W. believes) some time
after the writing of the *Republic*. The change, associated with
Plato's growing awareness of teleology in nature, consisted in asserting
the existence only of Ideas of natural objects, including their genera
and properties, and in denying Ideas of artefacta. The new view,
to be found in the later dialogues, is accompanied by a greater
stress on the *παραδεῖγμα* rôle of Ideas. The late dialogues, in their
discussions of the interrelations among Ideas and in their *διαιρέσεις*,
give the background of the next big change, the identification of
Ideas with Numbers. The theory of Ideas remains the kernel of the
doctrine and the Ideas remain separate real things ; but a supplement
is added to the theory. Finally, seeking the ultimate principles of
Numbers themselves, Plato developed the theory of the two *ἀρχάι*,
the One and the Indefinite Dyad.

Aristotle was still loyal to the theory of Ideas when he wrote the
Eudemus about 353 B.C. ; and when soon afterwards he wrote in the
Περὶ Τἀγαθοῦ a report on Plato's famous lecture he wrote as a faith-
ful disciple. In the *Περὶ Ἰδεῶν*, however, written at Assos not long
after Plato's death, Aristotle criticised Platonic doctrine, attacking
the theory of Ideas from the ' immanentist ' standpoint familiar to us
from his extant works and alleging an inconsistency between the
presuppositions of this theory and the doctrine of the two Principles.
In his further development Aristotle converted these Platonic

[1] W.'s book was finished in 1942 and he was unable to take account of
W. Van der Wielen's *De Ideegetallen van Plato* (1941) or of Harold Cherniss's
Aristotle's Criticism of Plato and the Academy, I (1944—see especially
pp. 223-305) and *The Riddle of the Early Academy* (1945). Among other
recent work may be mentioned articles by de Vogel (*Problems concerning
later Platonism*, Mnemosyne, 1949) and by Mansion (*La Critique de la
Théorie des Idées dans le De Ideis d'Aristote*, Revue Philosophique de
Louvain, 1949).

Principles into his own Form and Matter, abandoning the separate substantial Ideas.

As for the Academy as a whole, W. paints a picture of lively discussion and of give-and-take between this and the Platonic dialogues. The leading thoughts of the Academic proofs reported in the Περὶ Ἰδεῶν are to be found in the dialogues, but in the school they received their precise formulation. W. sees it as a proof of the ' geistige Freiheit ' of the Academy that Aristotle was able to publish a fundamental criticism of the theory of Ideas as a contribution to discussion which did not imply his immediate withdrawal from the Academy.

1. Περὶ Ἰδεῶν.

It is certain that much of Alexander's rich commentary on the first section of Aristotle's *Metaphysics* A.9 (the ' dialectical refutation ' of the theory of Ideas) is derived from the Περὶ Ἰδεῶν, to which he himself refers; and we know, too, from Alexander (92.27 ff.)[1] that the Περὶ Ἰδεῶν contained in detail the criticism of Eudoxus' view to which Aristotle alludes in *Met.* 991a, 17-19. W. goes further and supposes that it contained not only Academic arguments for the theory of Ideas and objections to them, together with criticism of certain variants on the orthodox theory, but also an exposé of contradictions between the theory and the doctrine of the One and the Indefinite Dyad. But his argument for this is weak. He contends that if the Περὶ Ἰδεῶν did *not* take any account of the Idea-Numbers theory and the two ἀρχαί it must have been written before Plato had developed these views—an unlikely supposition. Yet W. himself insists that the theory of Ideas always remained an essential part of Plato's doctrine ; and there is no reason why Aristotle should not, even after Plato's death, have devoted a monograph to it alone. The only real evidence for W.'s thesis derives from his interpretation of *Met.* 990b, 17-22 : ' And in general the arguments for the Forms destroy the things for whose existence we are more zealous than for the existence of the Ideas ; for it follows that not the dyad but number is first, *i.e.* that the relative is prior to the absolute—besides all the other points on which certain people by following out the opinions held about the Ideas have come into conflict with the principles of the theory.' (Ross's translation.) W. takes Aristotle to be referring to difficulties about the relation between Ideas and the One and Indefinite Dyad. But the passage can well be interpreted otherwise ;[2] and the fact that it is repeated in *Met.* M. 4, where Aristotle explicitly says (1078b, 9-12) that he is discussing the *original* theory without connecting it with the later Idea-Numbers doctrine is strong evidence against Wilpert's interpretation. The fact that he is following Alexander here is not

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[1] References to Alexander's commentary on the *Metaphysics* are to Hayduck's edition (*Commentaria in Aristotelem Graeca*, vol. I).

[2] See Cherniss's *Aristotle's Criticism of Plato*, p. 300 ff.

decisive for our problem since there is no good reason to regard this passage of Alexander as derived from the Περὶ Ἰδεῶν. I conclude, therefore, that the Περὶ Ἰδεῶν probably did not touch on the Idea-Numbers theory or the doctrine of the Principles. This conclusion carries no implication as to the date of composition.

W.'s exposition of the Academic arguments which Alexander reports in elucidation of Aristotle's cryptic remarks in *Met.* 990b, 8-22 is clear but does not, I think, contribute any new philosophical interpretation. Two of the Academic arguments deserve special mention here. The first (Alex. 81, 10-18) starts from a consideration of negative statements. When we say ' This is not white and that is not white ' we are denying (ἀποφάσκειν) to this and to that one and the same quality. This one identical quality is an Idea. By the same token when we assert that this and that *are* both white we are ascribing (καταφάσκειν) one and the same quality (= the Idea) to both the particulars. W. observes that the point of starting from negations is that while it may be a little plausible to say that the whiteness of one white thing is different from that of another it is highly unplausible to say that the whiteness a black thing has not got is different from that which another black thing (or a red thing) has not got. It is more noteworthy, however, that the train of thought presupposes the *formal* nature of negation (a fact that may be relevant in considering the status and functions of the all-pervasive Ideas of the *Sophist*). Aristotle, in objecting to the ἓν ἐπὶ πολλῶν argument that if valid it must imply the existence of Ideas of negatives, not-white and so on, seems to be ignoring or implicitly rejecting the correct view of negation taken by Plato and to be insisting on taking ' not-white ' as a straightforward predicate.

A further argument (Alex. 82, 11 ff.) starts by distinguishing three cases in which we may apply a common predicate to a group of objects. We may describe as men (*a*) a group of men, (*b*) a group containing one man and some pictures (or statues) of men, or (*c*) a group of pictures of men. Consider now the predicate ' equal ' as applied to pairs of particular things. Since phenomenal objects are always changing and no pair is ever perfectly (or ' really ') equal this does not correspond to case (*a*). Nor is it case (*b*) for no one phenomenal pair has any claim to count as the exemplar which other pairs only imitate. It remains that all particular pairs are correctly called equal only in the way in which pictures of men are correctly called men ; and we can infer the existence of a paradeigmatic, non-sensible Equal as the necessary condition of our correct application of ' equal ' to imitative phenomenal equals. This argument will be discussed below.

After stating the Academic arguments W. turns to the Aristotelian objections (summed up in *Met.* 990b, 11-15), some of which maintain that the Academic arguments do not prove the existence of Ideas at all (but only of what we may call immanent universals), others of which allege that the arguments if valid would prove the existence

of Ideas which the Academy denies. An important question is
raised by Aristotle's contention that the arguments ἐκ τῶν ἐπιστημῶν
(which say, roughly, that knowledge must be of something unchange-
able, *i.e.* of Ideas) must imply the existence of Ideas of artefacta.
This objection presupposes that at least some members of the Aca-
demy denied Ideas of artefacta, and it is certain from other evidence
that some Platonists in fact did so. But it is difficult to decide
whether Plato himself ever made such a change and, if so, what
precisely his reason was. W. argues that there was a change in
Plato's view (sometime after the *Republic*) and that thenceforth
only natural things along with their genera and properties were
credited with Ideas. He supports this view only briefly, his main
points being the following :

(α) Aristotle's remark διὸ δὴ οὐ κακῶς ὁ Πλάτων ἔφη ὅτι εἴδη
ἐστὶν ὁπόσα φύσει (1070a, 18) clearly implies that Plato made such
a restriction ; this is supported by Xenocrates fr. 30 (Heinze) and
D.L. III. 77.

(β) In Aristotle's *Protrepticus* (W. accepts Jaeger's conclusions as
to what parts of Iamblichus' *Protrepticus* can be referred to Aristotle)
a clear distinction is drawn between φύσις and τέχνη. The object of
φρόνησις is later said to be ἡ τῶν ὄντων φύσις καὶ ἡ ἀλήθεια (and
again ἡ φύσις αὐτὴ καὶ ἡ ἀλήθεια) ; and the object of φρόνησις in
the *Protr.* is in fact the world of Ideas. W. claims that the use of
ἡ φύσις as meaning the world of Ideas together with the previous
distinction implies that a change had occurred in the theory whereby
there were now said to be Ideas only of τὰ φύσει.

(γ) The term φύσις is especially prominent in Plato's later dialogues.

(δ) Plato came to believe that nature itself is teleological and that
human τέχνη imitates it. There is a resultant change in the theory
of Ideas : τέχναι have models in (phenomenal) nature, only τὰ φύσει
ὄντα have Ideas as models. There is now a greater stress on the
paradeigmatic rôle of Ideas.

This argumentation is inconclusive : (a) Aristotle's remark needs
closer consideration. It contradicts other passages where Aristotle says
that Plato sets up Ideas corresponding to all classes of αἰσθητά, and
the way Aristotle introduces it suggests that he is using a Platonic
quote to serve his own purposes without necessarily even wanting to
suggest that his application of it is the same as Plato's was. Words
like διό and γάρ must be treated in Aristotle—as in sources for
ancient history—as danger signals. They are constantly used where
an inference or construction of the author is being unostentatiously
put forward. Aristotle is a connoisseur of constructions and in them
he often takes odd phrases out of their context and lends them a
meaning they certainly did not originally bear. Doubtless this
procedure is sometimes due to negligence or misunderstanding and
sometimes it is based on the typically Aristotelian notion that people
often don't succeed in saying what they mean (or even in knowing
what it is that they really mean) ; but sometimes it constitutes a

somewhat esoteric joke and this may well be the case here if Plato
in fact made his remark—and was known to have made it—with a
quite different intention from that playfully ascribed to him by
Aristotle.

A convincing suggestion as to what Plato did mean is given by
Cherniss (*Aristotle's Criticism of Plato*, p. 252) in the course of an
exhaustive discussion of this whole problem.[1] He connects the
restriction ὁπόσα φύσει (as used by Plato) not with the antithesis of
nature and art but with Plato's insistence that in studying the world
of Ideas we must follow its natural articulation. This does imply a
certain restriction on the extent of the world of Ideas in that general
words which express ' casual ' conjunctions of properties will not be
proper names of Ideas. But the distinction between terms which
express genuine classification and those which must be treated as
mere shorthand for ' φ and ψ ' cuts across the distinction between
natural and artificial objects. (β) The evidence from the *Protrepticus*
is not strong. Iamblichus' *Protrepticus* is a hotchpotch of bits and
it is hardly fair to connect closely the passage in which φύσις means
nature as opposed to art with those in which φύσις is used of the
world of Ideas. In any case that the latter usage cannot be a result
of the former distinction is shown by the use made of the word φύσις
with reference to the Idea of table in *Rep.* 597 and in the κερκίς
passage in *Crat.* 389 (cf. also *Ph.* 103b, 5, where τὸ ἐν τῇ φύσει is
used to mean the objectively existing Idea at a time when Plato
certainly still believed in Ideas of artefacta). ' φύσις ' in fact meant,
inter alia, the real nature or character of a thing and so it inevitably
came to be used of Ideas without any implication as to whether or not
they were, in a different sense, ' natural '.

Moreover the distinction between φύσις and τέχνη is developed
in the *Protrepticus* in a way characteristic of Aristotle (*Physics* B)
but hard to reconcile with Plato's dialogues—including the latest.
The close Platonic parallel occurs in a statement of a materialistic
non-teleological view which Plato proceeds sharply to rebut (*Laws*
889). It is true that the purpose of the *Protrepticus* argument
is to emphasise teleology in nature; but it does so not as Plato
does by insisting that ψυχή or νοῦς controls nature, that nature
is θεία τέχνη ; but by an unplatonic contrast between φύσις and
τέχνη καὶ διάνοια—Plato's great point is that *both* nature *and* art
are controlled by διάνοια (*e.g. Soph.* 265c.). Divine art is, of course,
better than human (*Rep.* 381a, b) and it provides the materials that
human craftsmen use (*Soph.* 265e) ; human reason is itself derivative
from and less pure and capable than the divine (*Phil.* 30). But that
nature or God is the *supreme* craftsman by no means implies that
ordinary craftsmen do not work ' looking to Ideas '.

(γ) The frequent occurrence of the word ' φύσις ' in the later

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[1] Another possible interpretation of Aristotle's remark is given by R. S.
Bluck in CR., LXI, 1947, p. 75. (Cp. also now Ross's *Plato's Theory of
Ideas* p. 174, where Cherniss's view is adopted).

dialogues does not imply any limitation of the world of Ideas. The word has a variety of meanings all of which can be paralleled in dialogues of the early or middle period.

(δ) It has already been suggested that Plato's view of φύσις as θεία τέχνη and of ψυχή as in the truest sense τῶν πάντων αἰτία (*Laws*, 896d) does not carry any necessary implications about the extent of the world of Ideas and in fact rather indicates that human and divine reason both work in essentially the same way ' looking to Forms '. There is more specific evidence (leaving aside the controversial seventh epistle). The discussion in *Pol.* 292 ff. draws on the usual analogy with manual crafts in the course of stressing the need for absolute standards and pointing out that written rules are a poor substitute for the man who really knows these standards and can apply them to varying circumstances. *Pol.* 284d associates the concept μέτριον with τέχναι and it is pretty clear that the μέτριον type of ' measurement ' (283c ff.), like πέρας in the *Philebus*, is essentially connected with the Ideas (cf. *Gorg.* 503e, 504d). *Laws* 965b, c repeats the notion of the craftsman looking to the single pattern. *Tim.* 28a deduces that the Demiurge looked to Ideas from a general principle about the need to work to such eternal models if work is to be καλόν.

The first group of objections (summed up in *Met.* 990b, 11-15) contend, first, that the Academic proofs, if valid, prove the existence of Ideas which Plato denies, and, secondly, that they do not really prove the existence of any Ideas—as separate substances—but only show the existence of ' immanent universals '. Aristotle next refers to arguments of which (he says) ' οἱ μὲν τῶν πρός τι ποιοῦσιν ἰδέας, ὧν οὔ φαμεν εἶναι καθ' αὐτὸ γένος, οἱ δὲ τὸν τρίτον ἄνθρωπον λέγουσιν '. W. understands him to be mentioning here two difficulties Plato had himself seen, the first being that the instances of an Idea of a relative do not form a homogeneous class. Highly diverse things can be instances of Equal for example ; relative properties cannot be used as differentiae to define genuine genera or species. I shall not discuss this interpretation in detail (though I do not think that what Alexander says justifies it) ; but must express grave doubt about the implication that Plato ever regarded—or tried to map— the world of Ideas as a single hierarchy of the genus-species type. Platonic ' division ' certainly gave an impetus to classification work in biology and, in general, to the logic of genus and species. But did Plato ever hope or try to restrict the world of Ideas to those which fit into a single pyramidal system ? The fact, which W. admits, that Plato always retained Ideas of relatives suggests that for *his* conception of the world of Ideas the alleged difficulty about relatives was no difficulty. If it had been there would have been a similar trouble about many *non*-relational properties of which Plato always postulated Ideas. The *Sophist* seems to tell conclusively against W.'s whole notion.[1] There is no single Idea at the apex of a simple

[1] See Cornford's *Plato's Theory of Knowledge*, pp. 252-279.

'genealogical tree'. There are a number of Ideas with each of which all other Ideas 'communicate'. Moreover, 'communication' is not, as W. implies, a one-way relation between Species-Ideas and Genera-Ideas ; it is not one-way and it does not hold only between such Ideas. W. assimilates the Platonic ideal too closely to the Aristotelian. Plato's notion of charting the world of Ideas was not to draw one simple Porphyrian tree but to exhibit each Idea in all its relations to other Ideas. He would no more have been satisfied with a genus-species diagram than would a modern analytical philosopher, whose study of the logical grammar of a concept involves far more than establishing its place in a genealogical tree of concepts related in the genus-species way.

W. has a clear analysis of Alexander's account (83-85) of various forms of Third Man objection and rightly identifies that used in the Περὶ Ἰδεῶν with the one used in Plato's *Parmenides*. As elsewhere W. tries to make distinctions based on a perhaps over-sensitive interpretation of the text. He distinguishes an argument for Ideas as predicates (*i.e.* as 'explaining' how we can use one word for many particulars) and one for Ideas as models (which particulars imperfectly imitate). He understands Aristotle as insisting that both arguments lead to infinite regresses. The demand for a Predicate justifies that for a Super-Predicate ; and the postulation of a Model requires that of a Super-Model. Neither the text of Alexander nor Plato's dialogues justify the assumption that these two trains of thought were so clearly distinguished. In the *Parmenides*, it is true, one way of describing the relation of Ideas to particulars is tried as an alternative to the other. But the arguments for Ideas usually confuse together the 'common-predicate' and the 'paradeigm' jobs of Ideas. Indeed the possibility of an infinite regress criticism depends upon the identification of the Idea = Predicate with the Idea = Paradeigm (see Austin in *Proc. Ar. Soc.*, XVIII, 1939).

It is odd that Aristotle could produce the Third Man objection without—apparently—discussing the reasons why Plato felt his theory was not refuted by it. That Plato did feel this is proved not only by his continued adherence to the theory but also by his use of the same motif to prove the uniqueness of the Idea in *Rep.* 597c and *Tim.* 31a. Plato's defence would no doubt have been that while the Form Man *is* Man particular men only *have* Man ('have' being the name of a relation very difficult to understand or explain) and that Man and men must not be classed together as a group.

The relation between Man and men is for Plato analogous to that between men and imaginary men. Imaginary men do not constitute an extra sort of man alongside Frenchmen, Italians, etc., yet they are not just instances of any of these ordinary sorts of men. This, the problem of 'seeming to be', was dealt with by Plato by means of a status-distinction between two *types* of object. Similarly the

difference between Man and men is not an ordinary difference of
kind (such that they could be classed together as instances of some-
thing), but involves a difference of ontological type, level or status
which makes it impossible (logically improper) to regard Man and
men as constituting a class. The Academic argument quoted above
which distinguishes three cases of common predication is worth
recalling in this connexion. It says, in effect, that the word ' man '
as ordinarily used is an umbrella-word that covers seriously different
cases. Let ' man(1) ' mean a real live person and ' man (2) ' a
likeness of a man (1). Then when we lump people and paintings
together as men we are liable to overlook the fact that people are
men (1) and paintings are men (2)—a fact vital for metaphysics
because different levels of reality are involved. It must be highly
improper to draw ontological conclusions from talk in which ' man '
is left unanalysed. But this is just what the Third Man objection
does. To explain a lot of men (1) the theory postulates a Form—
' man (F) '. Man (F) and men (1) can no more be thrown together
as a class than can men (1) and men (2). In each case such a
procedure is rendered improper by the existence of a difference in
type or level. This difference the Third Man neglects. It is not
unreasonable to suppose that Plato, in rejecting the Third Man as
an objection, was relying on his insight into a fact about the logic
of types.[1]

The Academic proof, following the classification of kinds of pre-
dication, that phenomenal particulars are all copies of Ideas is
faulty, notably in that while the initial classification takes for
granted that many particulars (though varying in certain ways)
may nevertheless all be strictly described as X the argument infers
from variations in particulars that they *cannot* all be strictly described
as X and finally concludes that *no* particular can ever be strictly
described as X. The choice of examples (' man ' for the initial
classification, ' equal ' for the subsequent argument) tends to conceal
the flaw—for we would insist that particular people really are men,
while we might feel like saying that no two particulars are ever
really equal. It might indeed be suggested that the whole argument
is based on a distinction between different kinds of concept and that
it ' proves ' Ideas for ' ideal limit ' concepts only on the presupposition
that there are none for other concepts. Plato, however, doubtless
regarded ' man ' too as an ideal limit concept. The fault in his
argument is thus comparable to that of those who have started by
presupposing that we do sometimes *know* we are seeing a dog or a
fox and have finally deduced that we *never* really know such a
thing.

In his last section on the Περὶ Ἰδεῶν W. discusses the difficulties
about Idea-Numbers and the Principles which Alexander (85.15 ff.)

[1] And of course on his own metaphysical views. For phenomenal
particulars as ' really ' shifting images or reflections see especially *Tim.*
49-50.

raises and which W. wrongly, I think, believes to have been contained in the Περὶ Ἰδεῶν. One of the main points he attributes to Aristotle is that the whole mode of analysis or explanation adopted in the quest for Principles is different from and to some extent incompatible with that on which the theory of Ideas is based.

2. Περὶ Τἀγαθοῦ.

The backbone of the second part of the book is the long passage in which Sextus Empiricus reports (as Pythagorean) the view that numbers are the principles of everything and that they are themselves derived from the ἕν and the ἀόριστος δύας (X. 248-284). The passage has normally been regarded as a neo-pythagorean blend of Platonist and Pythagorean views, but W. (in *Hermes*, 76) claimed it as in the main based on Aristotle's record of Plato's famous lecture. He did this partly on the basis of parallels (in thought and expression) which he found in Plato and in early academic works like the *Divisiones Aristoteleae*, partly by comparing one large chunk of Sextus with a passage preserved from a book on Plato by his pupil Hermodorus and with a passage of Alexander in a context where he refers to the authority of Aristotle's Περὶ Τἀγαθοῦ. He regards the three passages as clearly identical in inspiration, the Sextus one being by far the fullest and therefore specially valuable. He argues that the *whole* of Sextus' report is a clear unity except for a few discoverable late insertions, and with it and the other evidence (mainly from Alexander) he reconstructs in some detail the content of Aristotle's work and the nature of Plato's later doctrines.

It will first be useful to indicate the content of the Sextus passage :

X. 248-262. The true φυσικός must analyse things as far as possible and not be satisfied with a reduction of perceptible bodies to imperceptible (*e.g.* atoms). Just as we analyse syllables into letters so we must pass from the corporeal to its incorporeal στοιχεῖα. Platonic Ideas will not do as the ultimate principles of things ; each taken by itself is said to be one, κατὰ σύλληψιν δὲ ἑτέρας ἢ ἄλλων δύο καὶ τρεῖς καὶ τέσσαρες—number is thus prior to them and an Idea is described as one, two, etc., in virtue of its sharing in number. Similarly, mathematical solids are not ultimate : planes are prior to solids, lines to planes, and a line presupposes the number two (for it lies between two points). Numbers in turn derive from the One and the Indefinite Dyad.

X. 262-276. A further deduction of these two supreme principles follows from a classification of things into the classes τὰ καθ' ἑαυτά (horses, plants, etc.), τὰ κατ' ἐναντίωσιν (good-bad, moving-stationary) and τὰ πρός τι (above-below, larger-smaller). All substances fall under the One. Of each pair of contraries one does not and the other does admit of a difference of degree. The former type (good, stationary) falls under the Equal and so mediately under the One, the latter kind falls under the Unequal and so under Excess and Defect and finally under the Indefinite Dyad. All τὰ πρός τι fall

under the Indefinite Dyad. Thus the supreme genera or principles of all things are the One and the Indefinite Dyad.

X. 276-284. From these principles are derived numbers and then magnitudes (the point corresponding to one, the line to two and so on) and the perceptible universe.

W. analyses these trains of thought at length and supports by parallels his ascription of them to the Περὶ Τἀγαθοῦ and indirectly to Plato. His interpretation of the Idea-Numbers theory starts from the phrase in Sextus κατὰ σύλληψιν δὲ ἑτέρας . . . (see above). This he takes to refer to the increasing number of contents or determinations involved as one moves downwards in the Porphyrian tree of Ideas from the apex, which is τὸ ἕν. A given Idea contains as contents all those Ideas which come above it in the pyramid of Ideas as its genus, genus of genus, etc., up to and including the apex Idea. It further contains one additional content so that the number it contains in all is one greater than that of the Idea immediately above it. According to W., Plato's problem in the later dialogues with regard to the one and the many in the world of Ideas is the question how a Species-Idea, though a unity, nevertheless contains other Ideas—its Genus-Idea and so on upwards. Plato came to the view, implied in these dialogues and stated in the Idea-Numbers theory, that it is the *essence* of Ideas to be synthetic unities, and that as such they *are* numbers. For a number is simply and solely a synthetic unity. *What* number a given Idea is depends on the number of contents in it, or—in other words—on the number of steps needed to reach it if one works down from the apex-Idea, the One. A single number can express the structural essence of several Ideas (each of which *is*, therefore, this number), yet the number is not for this reason on a higher level than the Ideas, for it exists only *as* Ideas. Plato next asked how numbers themselves could be synthetic unities and so was led to the doctrine of the two Principles. W. ends by adding his own suggestion to those already made by scholars as to the way in which Plato may have ' derived ' numbers from the One and the Indefinite Dyad.

All this is expounded with erudition and with a refreshing clarity in strong contrast to the dark style of Stenzel's *Zahl und Gestalt,* by whose doctrine however W. is obviously influenced. I can do no more here than indicate the nature of the objections to which I think W. is exposed. And firstly, his general approach to Aristotelian evidence seems somewhat uncritical. We *know* from extant works that Aristotle regularly alternated between different ' interpretations ' of his predecessors in order to bring out contrasts or similarities with his own views. We *know* that he often states what *he* regards as the necessary implication of some philosopher's view as if he is in fact reporting beliefs actually held and asserted by that philosopher himself. Clearly one must consider *all* that Aristotle says on a given point, and his philosophical motives for saying it, before one can begin to infer back to the doctrines really held by other people.

W. does indeed contend that the $\Pi\epsilon\rho\grave{\iota}$ $T\grave{\alpha}\gamma\alpha\theta o\hat{v}$ *must* have contained a reliable report of the lecture, since Aristotle wrote it while still a supporter of Platonism and to be read by fellow-disciples. But the obscurity of the lecture (or, perhaps we should say, the fact that it wasn't understood) was notorious. Other pupils of Plato thought it worth while to publish *their* accounts of it. What *Plato* really meant, even on fundamental questions, continued to be disputed in the Academy ; no one could produce authoritative answers, but all relied on inferences and interpretations (as in *Met.* M and N), including inferences, sometimes mistaken, from the dialogues. Under these circumstances a naïf approach to Aristotelian evidence is dangerous. Nor would it be enough to show parallels between Aristotle's reports on the unwritten doctrines and late Platonic dialogues. For the existence of such parallels would be quite compatible with the view that Aristotle's account of the enigmatic lecture was in fact a highly personal and possibly mistaken *interpretation* of it—Aristotle would naturally have used his knowledge of the dialogues in constructing his interpretation. Moreover, it is necessary to take account of *all* the evidence not only in assessing the reliability of what Aristotle said in the $\Pi\epsilon\rho\grave{\iota}$ $T\grave{\alpha}\gamma\alpha\theta o\hat{v}$ but also in determining what he said there ; for it is improbable that this work contained a clear statement of some view about which Aristotle elsewhere admits to having no good evidence, or that it developed any important line of thought nowhere mentioned in extant Aristotelian works. (I do not think that a reader of the *Metaphysics* would ever suspect that Aristotle had the conception of the Idea-Numbers which he must have had *if* he had written the $\Pi\epsilon\rho\grave{\iota}$ $T\grave{\alpha}\gamma\alpha\theta o\hat{v}$ which W. reconstructs). W. gladly admits that much more must be done before his conclusions about Plato can be regarded as firmly-established, but he does not seem to be aware, even within the field of his present enquiry, of the peculiar difficulty of handling Aristotelian evidence.

Secondly, the Sextus passage seems more suspect than W. allows. The use in crucial places (especially X. 255, 258, 263) of distinctively late terminology ; the whole framework of the analysis, which speaks as though material things constitute the whole of the phenomenal world to be explained ; and the argument of X. 262-276, which seems not to cater for Ideas at all and which goes against the normal Aristotelian (and Alexandrian) account of late Platonism, whereby all Ideal and phenomenal things participate, directly or indirectly, *both* in the One *and* in the Indefinite Dyad ; these and other more detailed considerations (*e.g.* X. 265 on ' double ' and ' half' contradicts Alex. 56.26) make it more prudent to regard the Sextus report as a late blend of materials from different sources than as a touched-up but basically accurate transcription from the $\Pi\epsilon\rho\grave{\iota}$ $T\grave{\alpha}\gamma\alpha\theta o\hat{v}$. Of the ' parallel ' passages, the Hermodorus one need not belong to the context of the Idea-Numbers theory at all ; it mentions neither these nor the Indefinite Dyad. Alexander 56, 13-20 W.

interprets so that it corresponds to Sextus X. 262-276 ; but it can more easily be interpreted so that it conflicts with this already suspect Sextus passage.

W.'s claim that his Idea-Number theory is already contained implicitly in the late dialogues seems hardly justified. Apart from the already-mentioned difficulties concerning the pyramid notion of the world of Ideas, Plato's use of περιέχειν and his statement that we must not just say of a given generic Idea that it is one and many but must ask *how many* it is (*i.e.* how many subordinate Ideas it ' contains ', *Phil.* 16d) imply that for Plato the higher Idea is fuller, not emptier, than the lower. Again, the problem of the one and many in the world of Ideas is not simply concerned with genera and species, but is the extension to this field of the whole question of predication. This question demands (and receives) a more subtle analysis of relations among Ideas than is implied in W.'s account of the Idea-Number theory.

Finally, I do not think that the theory W. attributes to Plato is very illuminating, if indeed it is intelligible. Aside from difficulties about the actual structure of the pyramid of Ideas (W. for instance does not say whether Ideas of relations can be fitted in), how can we be satisfied to accept, say, the number Four as *being* the Ideas of Body, Justice and Opinion (because it expresses the synthetic unity which is their essence), while admitting that Body, Justice and Opinion are different one from the other ? Surely all differences among Ideas must be real differences; they are at once admitted and denied in W.'s theory.[1] Again, W.'s procedure for ' deriving ' the numbers from the Principles (based, like Van der Wielen's,[2] on the method of progressively dividing a line mentioned in Aristotle's *Physics*, 206b 3 ff. and by Porphyrius *apud* Simplicius, *Physics*, 453.36 ff.) does not do justice to the Platonic doctrine of ἀσύμβλητοι ἀριθμοί.[3] Each number is conceived as an aggregate of parts ; and though in the case W. gives (p. 217, n. 41) each part of one number is ' specifically different ' from any part of the other, this will not, if I understand the procedure, be true in all cases ; eight will be represented by a line divided into eight equal parts, while seven will be seven of those same parts.

I trust that to have taken this book seriously and to have criticised it freely is the best way of saying that it is an important and stimulating study, which will doubtless be the starting-point for much discussion and further enquiry.

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[1] It seems altogether too high-handed to say (p. 170) that the (theoretical) generation from the One of numbers (which W. quaintly describes as developing in a genus-species way as do the Ideas) is at the same time the generation of the corresponding Ideas. ' Mit der Zahl sind die ihr entsprechenden Ideen gegeben'. This leaves much unexplained.

[2] *Op. cit.*, p. 118 ff.   [3] See Cook Wilson's article in CR, XVIII, 1904.

8

*The Development of Aristotle illustrated from the Earliest Books of the
Physics.* By H. E. RUNNER. Kampen : J. H. Kok. Pp. 160.

THIS is a puzzling work, the merit of which it is difficult to assess.
The author begins with a clear and thoughtful account of the present
state of the study of Aristotle's development, giving in particular
a critical summary of the work of Jaeger and Nuyens, in which their
value and limitations seem to be fairly and usefully set forth.   One
looks forward to an equally clear and helpful exposition of Dr.
Runner's own contribution, but here we are met with the " charting "
of philosophical conceptions undertaken by Professor Vollenhoven
of the Free Reformed University of Amsterdam, which forms the
framework of the present thesis.   Perhaps no fair review of this is
possible without a first-hand acquaintance with the Dutch text of
Vollenhoven.[1]   On the other hand, Runner is at pains to forestall
this difficulty by a section of fourteen pages expounding the ideas
and terminology of Vollenhoven's work in so far as they are necessary
to an understanding of his own.   Aristotle's philosophy is to be seen,
rightly of course, as developing out of that of his predecessors, and
is therefore to be examined for traces of this or that conception into
which Vollenhoven has classified pre-Aristotelian philosophy.
Here our troubles begin, for the classification is a new and highly
complex one, and unfortunately in the summary of it here given very
few philosophers are mentioned by name.   When they are, the
principle of classification is not always clear.   Anaximenes appears
as a partial universalistic subjectivist of the non-contradictory type.
Subjectivism, we have been told, " refuses to recognize anything but
subjects in the cosmos ", and some may find it difficult to recognize
Anaximenes here.   An example of the subsequent application of
this classification to Aristotle is the conclusion (pp. 71-73) that
Phys. VII exhibits a realistic cosmogono-cosmological partial
universalism of a monistic variety, the monism being of a semi-
contradictory type.

This sounds like a parody, and it must be repeated that the author
has done his best to explain the use of these uncouth terms.   Al-
though obscurities remain for the uninitiated, it is not on that ground
that the method seems most open to criticism.   It is rather that to
force each previous philosopher, and each stage of Aristotle's philo-
sophy, into such a pre-fabricated framework of modern concepts is
inevitably to create divisions among them which have no more than
a semi-reality in the thought of the sixth to the fourth centuries B.C.
When one considers in addition how little is known of some of the

[1] *Geschiedenis der Wijsbegeerte : eerster Band, Inleiding en Geschiedenis
der Griekesche Wijsbegeerte voor Platoon en Aristoteles.*   Wever, Franeker,
1950.

men mentioned, the attempt seems even more hazardous. What evidence can there be, one wonders, for attributing to Metrodorus of Lampsacus an impetus theory of motion and a vitalistic (as opposed to instrumentalistic) conception of the biotic (p. 47) ? In fact this " philosophical historiography " (the author's description) seems at times scarcely historical enough, though " unhistorical " is a favourite criticism with Dr. Runner himself. Much of the book talks in terms so far removed from contact with any ancient philosophical text that it is almost impossible to bring the two into focus simultaneously.

The aim of the work is, by applying Vollenhoven's method to certain parts of the *Physics*, both to test the method and to find a securer basis for the chronology of the books concerned. An analysis of Aristotle's argument in each book is followed by a brief statement of the philosophical themes and a discussion of them divided into " background " and " foreground ". These mean respectively, in Vollenhoven's terminology, the law for the cosmos and the cosmos itself, or as we might say, the cosmology (foreground) and its philosophical presuppositions (background). The parts treated are (in this order) Books VII, I, II (omitting chapters 4-6), V and VI.

Of Book VII Dr. Runner asks first (" background ") whether Aristotle still believes in the Ideas, and decides in the affirmative. He follows Cherniss in rejecting Jaeger's argument from 249b, 23-24, on the ground that the identification of essence and numbers there is not Platonic but Pythagorean.[1] He himself points to 247b, 1–248a, 6, arguing that the way in which the noetic part of the soul is carefully guarded from any sort of motion presupposes the Platonic view of an intelligible world behind changing phenomena, which can only be apprehended by something changeless in the knower. Moreover, when Aristotle speaks of $\tau\grave{o}\ \nu o\eta\tau\iota\kappa\grave{o}\nu\ \mu\acute{\epsilon}\rho o\varsigma$, he shows himself to adhere to a particular one of three Platonic views of the soul, represented in a middle group of dialogues opposed both to an earlier one (*Phœdo* and—most surprisingly—*Philebus* [2]) in which soul and mind coincide, and a late one (*Critias* and *Laws*) in which they are entirely separate. Moreover the passage (247b, 18-248a, 3) suggesting that we do not acquire new knowledge, but recover knowledge lost in the violent motions of birth and childhood " is Platonic realism expressed in a manner worthy of the master

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[1] Ross's new (1951) book on *Plato's Theory of Ideas*, with its critical attitude towards Cherniss, may lead Dr. Runner to some modification of his views here.

[2] To regard the *Philebus* as anything but one of the latest of Plato's dialogues, as is done here *en passant* without comment or argument, is, of course, to go against the opinion of every scholar who has concerned himself with the order of the dialogues. See Ross's table of the views of von Arnim, Lutoslawski, Raeder, Ritter and Wilamowitz (*Plato's Theory of Ideas*, p. 2), and his note (*ibid.* p. 9, n. 1) pointing out that in addition Baeumker, R. G. Bury, Poste, Robin and Taylor have placed it after the *Timaeus*.

himself ". To see in this passage reference to a recovery of knowledge, in the sense of Platonic ἀνάμνησις, is perhaps to read too much into the Greek. As the Loeb editor says, the implied point of the sentence describing the acquisition of the state of knowledge is that it is not a process of qualitative modification, though it *involves* a modification of bodily organs. This exactly describes the act of sensation by the soul in the *De anima*, where the distinction between physical organ and psychical faculty is clearly drawn and the point is made that disturbance of the organ may interfere with the soul's awareness.[1] The parallel is close enough to make it questionable whether Aristotle has radically changed his view about the acquisition of knowledge as a whole, or whether the motionless state of the knowing soul necessarily involves the assumption of a Platonic world of intelligibles.

For the " foreground " of Book VII Runner draws largely on Vollenhoven's terminology. " Partial universalism " (which " teaches the original existence of both universals and particulars alike ") is inferred from 247b, 4-7, especially ὅταν γὰρ γένηται τὸ κατὰ μέρος, ἐπίσταταί πως τὰ καθόλου τῷ ἐν μέρει, and this is said to correspond to Plato's position during his " school-forming years ". Yet it seems doubtful whether any precise chronological indications can be drawn from the words, when one considers how closely the doctrine resembles that of *An. Post.* (*cf.* 100a, 16, καὶ γὰρ αἰσθάνεται μὲν τὸ καθ᾽ ἕκαστον, ἡ δ᾽αἴσθησις τοῦ καθόλου ἐστιν) and of Aristotle's mature thought in general.

The " cosmogono-cosmological " nature of this stage is argued from the use of the terms σύγκρισις and διάκρισις in 243b, 11, which are given cosmogonic significance as, *e.g.*, in Empedocles and Anaxagoras. But to do this, it is necessary to reject Simplicius's reading ἤ for ἢ in 243b, 11, adopted by Prantl, Carteron and Ross. " Only the reading of the manuscript makes sense ", says Runner, ignoring the fact that Simplicius gives a reasoned interpretation which, whether right or wrong, certainly makes sense.

" Monism " is argued from two passages : (*a*) a detail in Aristotle's refutation of Zeno's argument about the grain of millet (250a, 24), where he says that the part of a whole has no separate existence, but only exists, as itself, potentially. " This is pure monism ", says Runner, for it means that " the whole exists originally, before the parts. The parts exist in the whole only potentially, that is, secondarily, after a process of division ". There is in fact no statement that the whole exists originally, before the parts, nor have the words any cosmological significance. As Simplicius says, the potential existence of the parts is limited to the time ὅτε ἔστιν ὅλον. They may have existed actually before, and will again. The sole point of the argument is that, when we are considering, say, a heap as a whole, we must look upon its component parts solely *as* parts, and as having only potential existence as separate entities. Awareness of this will save us from Zeno's fallacy. Nothing is implied

[1] *De an.*, B, 424a, 17 ff.

about chronological priority, nor is any application to the cosmos intended.

(*b*) 249a, 22. Ross does not " make nonsense of this ", as Runner claims. The passage means no more than is clearly expressed in the Loeb translation : " The genus is not indivisible, but besides the genus, there are a number of varieties concealed in it ". For Runner the passage is evidence of a " monistic theme ", and part-proof that *Phys.* VII was composed after Plato's *Theaetetus, Sophist* and *Politicus* but before the *Laws*. Similarly, on p. 80, he argues that " in the *Sophist* of Plato there is the same interest in genus and species that we see in *Phys.* VII ". But this interest, as displayed in the passage quoted, remained with Aristotle all his life, and of course became particularly important in his later work on biology.

On p. 75, with some idea of bringing *Phys.* VII into connexion with the " school " of Anaxagoras, Runner seeks a reference to an impetus-theory of motion, and finds it in 243a, 32-3 (Ross's numbering) : τὸ δὲ πρῶτον κινοῦν, μὴ ὡς τὸ οὖ ἕνεκεν ἀλλ' ὅθεν ἡ ἀρχὴ τῆς κινήσεως, ἅμα τῷ κινουμένῳ ἐστί. Here " the notion of a prime movent as final cause is explicitly rejected for one described with the words ' whence the beginning of motion comes ' ". Since in the next sentence it is claimed that the reference to a final cause is a later, and possibly un-Aristotelian, addition, it is difficult to see how we are to regard it as " explicitly " rejected. Moreover, if it is to go, so must the whole parenthesis with the reference to an efficient cause. But the truth is that, here again, Aristotle is giving nothing away about his views on the ultimate cause of motion. He is simply inserting the obvious proviso that the contiguity of mover and moved only applies when " mover " is used in the sense of efficient, not final, cause. Runner's contention appears to lead him to the strange conclusion that the theory of an external mover in Aristotle preceded that of motion as immanent in things themselves.

A little later (p. 78), Runner takes τὰ μὴ αἰσθητικὰ τῶν μερῶν, (244b, 9), which he correctly translates as " the parts not capable of sensing ", to refer to the part (singular) of the soul capable of thinking. This is perverse, and the whole sense of the context suggests that it has the natural meaning of those parts of the body (*e.g.* hair) which are incapable of sensation.

After " the *Physics* of the Platonic years " comes " the *Physics* of the earliest non-Platonic years ", divided into the anthropological, zoological and phytological theories of interaction (Books I, II and V plus VI respectively). The application of these terms to Aristotle's writings, in accordance with the explanation of them given on p. 44, is not made at all clear. The discussion of Book I (pp. 99 ff.) begins by describing it as characterised by a recognition of objects, and so opposed to the " subjectivist " school. One clear point is made (p. 100), that the statement ἡ ὕλη ἀριθμητή (190b, 25) means " matter is generally countable " (p. 105), thus betraying " strong

Pythagorean influence " and associating Aristotle with the " mathematical objectivists ". Here the unjustifiable generalisation of a particular argument has led to a complete distortion of the meaning. As Ross (*ad. loc.*) says, the statement should be taken as a defence of the expression ἀριθμῷ ἕν just above. Matter is numerically one, for it is the matter as a whole that must be counted (*e.g.* bronze plus privation-of-a-statue), in spite of the possibility of logical division of it into matter and privation. The privation is only an accident. This seems in fact to be a repetition of the regular Aristotelian tenet that matter is the principle of plurality or individuation, which has no specifically Pythagorean affinities.

In omitting chapters 4-6 from the discussion of Book II, Runner says that they merely interrupt what is otherwise a continuous, congruous discussion on the subject of causes. This is not so. In chapter 3 Aristotle had enumerated his four types of cause. At the beginning of chapter 7 he claims that these are the only four. This he could not have done if the discussion of chance in chapters 4-6 had not taken place, since the objection might immediately be raised : " What about chance as another kind of cause ? " (λέγεται δὲ καὶ ἡ τύχη καὶ τὸ αὐτόματον τῶν αἰτίων, chapter 4 *init.*) By proving in these intervening chapters that chance is no independent cause, but represents only a particular (*viz.* incidental) mode of operation of the efficient cause, he has disposed of that objection and can make his claim.

Discussing the question of realism in Book II, Runner regards Aristotle as maintaining the distinction of κόσμος νοητός and κόσμος ὁρατός on the basis of 193b, 33-35 and 194a 1. This is surely misleading. Aristotle there says that the objects of mathematics are χωριστὰ τῇ νοήσει κινήσεως. They are " separable in thought ", *i.e.* in thought only, not in reality. This is the opposite of the realist belief that they have independent existence in a separate world of which thought makes us aware.

On pp. 122-4 there is a discussion of nature as an immanent principle of motion which is refreshingly clear and free from jargon. Yet there is perhaps more to be said for the view quoted from Ross that nature is not necessarily an *all-sufficient* cause of motion even in this book. I doubt if Aristotle at any stage believed in the development or movement of organic objects (or indeed of any objects) towards their proper form without the existence of some perfect reality under the influence of which the growth and movement of nature takes place. Probably he would at all times have regarded the phrase " immanent teleology " as nonsense.

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VII.—NEW BOOKS

In Defence of Philosophy against Positivism and Pragmatism. By MAURICE
   CORNFORTH. London : Lawrence and Wishart, 1950. Pp. xv + 260.
   12s. 6d.

MR. CORNFORTH is already the author of a philosophical work in the Marxist
manner published in 1946 with the title *Science versus Idealism.* In that
book he developed Engels's view that the history of philosophy has always
been a struggle between Idealism, which is religion in disguise, and Material-
ism, which is the champion of science. Much of the book was criticism of
logical analysis and logical positivism as presented by Wittgenstein in
the *Tractatus* and by Carnap in *The Logical Syntax of Language.* He
concluded that, in spite of the former's use of the Principle of Verifiability,
and in spite of the latter's Physicalism, both these philosophers were
disseminators of Idealism and therefore fundamentally opposed to science
and progress. *Science versus Idealism* has now been published in Russia
with a preface by Professor Alexandrov, who must be unaware of or have
charitably forgiven Mr. Cornforth's strictures in the *Modern Quarterly* on
his ill-fated *History of Western European Philosophy.* Mr. Cornforth is thus
ousting Dr. John Lewis from the position of chief exponent in Great
Britain of the philosophy of Marxism-Leninism. In a penetrating review
in MIND (1947) Mr. Rush Rhees called attention to some important short-
comings in Mr. Cornforth's understanding of the philosophers he was
claiming to refute, and a reviewer in the *Modern Quarterly* expressed some
doubts about the simple " two camps " view of the history of philosophy
which Mr. Cornforth had taken over from Engels. Mr. Cornforth shows no
signs, in *In Defence of Philosophy,* of having attended to Mr. Rhees's
objections, but he is, perhaps, slightly less obsessed by the " two camps "
theory, though he still canvasses such notions as " the philosophers of the
Catholic camp ". Furthermore, the Introduction to the present book
shows that the author is somewhat more aware than before of the kinship
between the Positivist and the Marxist attacks on *a priori* speculation.
In Defence of Philosophy comprises a positive defence of Dialectical
Materialism along with criticisms of Russell, the Vienna Circle, James,
Dewey, Ayer, Carnap's *Introduction to Semantics* and *Meaning and Necessity,*
Morris's " semiotic ", and some of the popularisers of semantics.

   Throughout the book Mr. Cornforth writes in the truculent and phari-
saical manner characteristic of so much Marxist literature, but this should
not be allowed to conceal the fact that he makes some pertinent criticisms
of certain of the views he discusses. Thus, however it may be dressed
up as an *analysis* of Common Sense, Phenomenalism is opposed to it, and
Mr. Cornforth gives apt quotations from philosophers claiming to be anti-
metaphysical empiricists which show that they conceal a dim sort of
metaphysics behind their façade of sense data (or sense contents) and
constructions. A reader unacquainted with the methods of Marxist
controversy might, however, have expected a Marxist philosopher to find
and welcome points of agreement with Dewey's instrumentalism, and, in-
deed, with Pragmatism generally. But Mr. Cornforth sets out to show that
Dewey is a subjective idealist with a " long career as a philosopher of im-
perialism " (pp. 212-213). He criticises Dewey, as he is entitled to do, and

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as others have done, for pursuing his arguments against the given to an extent that appears to conflict with the objectivity of the physical world. He runs into confusion by quoting Dewey on p. 170 as saying " An organism does not live *in* an environment ; it lives by means of an environment ", and then himself saying on p. 173, as if it were a criticism of Dewey rather than something that Dewey himself insists on : " For men, in virtue of human labour, do not just react to a given environment but consciously change the environment and in many respects consciously produce their own environment." This, however, is the sort of mistake that any honest but over-zealous controversialist might fall into. But when, on pp. 170-171, Mr. Cornforth criticises Dewey for emphasising the biological rather than the social origins of thinking, his method takes on quite another form.

> " In this way ", he writes, " Dewey's 'naturalism', which leads him to speak of the ' biological matrix ' of thinking, leads him to render obscure the real social matrix of thinking, and to treat human social activity and social relationships as biological activity and biological relationships. By parading this obscurantism as ' natur- alism ' he seeks to give it a ' scientific ' and ' progressive ' appearance. But it is not scientific but obscurantist, nor progressive but profoundly reactionary.

> " It is, just this type of ' naturalism ', the ' biological ' view of man and of human activities and relationships, which in other con- texts manifests itself in racialism and in the social doctrines of eugenics : there is a direct link—none the less direct because not obvious on the surface—between Dewey's philosophic ' naturalism ' and those forms of man-hating, pogrom-mongering reaction, which, together with ' naturalistic ' philosophy, are quite widespread in the U.S.A."

It is difficult to decide which is the more disagreeable feature of the passage—the cunning with which it stops short of saying that the great American liberal has supported racial persecution, or the skill with which it suggests that he has. This is no isolated instance since, quite apart from the numerous places in which non-Marxist philosophers are, with no justi- fication whatever, accused of " trickery ", there is, on pages 242-243, a similar attack on Popper. After saying that at present " an ideological crusade against communism is being whipped up throughout the world ", and that "leading empiricists are to be found in the vanguard ", Mr. Cornforth gives a summary of *The Open Society and its Enemies* in seven- teen lines. In this summary, among other misleading descriptions of Popper's views occurs the insinuation that the contrast Popper draws between " open " and " closed " societies is merely a contrast between western democracy and totalitarian *communism*, rather than a contrast between liberal societies on the one hand and *all* forms of totalitarian society on the other. Nothing is said of Popper's criticisms of non- Marxist totalitarianism, and nothing of his detailed and by no means unsympathetic discussion of Marx's social philosophy. But immediately after this garbled " summary " occurrs the following paragraph :

> " In this way does contemporary positivism embrace the con- temporary imperialist conception of the struggle to the death of " western democracy " versus " communist dictatorship "."

It is, of course, a principle of Marxism-Leninism that philosophy should be written in a " party spirit ", with " partyness ". This principle, which

originated with Lenin, was stressed a few years ago by the late Andrei
Zhdanov in his speech to a gathering of Russian philosophers on the occasion
of his condemnation of the third volume of Alexandrov's *History of Western
European Philosophy* for its " passive, meditative, academic character ".
Evidently Mr. Cornforth has taken this advice so thoroughly to heart that
in avoiding passivity and meditativeness he actively spreads misrepresen-
tation in a manner that no instructed person could possibly describe as
academic.

H. B. ACTON.

In Defence of Reason. By H. J. PATON. London : Hutchinson's
University Library, 1951. Pp. ix + 288. Price 16s.

IN this volume Professor Paton has collected fourteen essays and addresses
written by him during the last quarter of a century. All but two—
' Existentialism as an Attitude to Life ' and ' Kant's Views on Inter-
national Law '—have appeared in print previously. Half-a-dozen of the
papers deal directly with fundamental doctrines in the philosophy of
Kant ; but in most of the others also (as their author would be the first
to acknowledge) a Kantian inspiration is manifest. Perhaps the sole
exception is an early paper, ' Plato's Theory of *EIKAΣIA* ', in which Paton
ingeniously adapts Croce's account of the aesthetic activity of spirit to
the elucidation of the first segment of Plato's Divided Line. The book
takes its title from no single paper, but rather from ' a certain unity ' in
the papers as a whole, in that ' they are concerned, directly or indirectly
with the defence of reason . . . as that power of general intelligence which
distinguishes man from the brutes and can be displayed in action no less
than in thought '. The point of the title might, I think, be brought out
a little more explicitly by saying that the papers aim at substantiating
and exemplifying in various ways their author's conviction that the human
mind has the power of grasping objectively necessary principles alike
in the world of facts and in the world of values.

The Kantian papers reprinted here are of broad design, and seem well
calculated to induce second thoughts in those who assume that essays in
Kantian interpretation are of interest only to Kantian scholars, with little
independent philosophic relevance. It is hard to see how such an attitude
could survive the careful reading of papers like ' Kant's Analysis of Ex-
perience ', or ' Kant's Idea of the Good '. Paton's long preoccupation with
Kant has never been that of the mere scholar. He is attracted to the
study of Kant primarily because he believes that, the better we understand
Kant, the clearer it becomes that he is talking quite exceptionally good
philosophic sense. In his Preface Paton expresses a modest hope of being
able to show that ' some of his [Kant's] doctrines . . . are still worthy of
consideration as setting forth possible methods for the solution of our
present problems '. Readers of the essays that follow are unlikely to deny
that at the very least this much is achieved.

Perhaps the most valuable item in the whole collection, however, is
the paper on ' Self-Identity ' (MIND, 1929). Its main contention—that
theories for which a mind can be analysed without remainder into a series
of events related in accordance with certain principles do not even begin
to account for the unity of the mind as *knower*—seems to me to be es-
tablished unanswerably by arguments that are throughout masterly in
their economy and point. This essay is, I think, much less widely known

than it deserves to be. The considerations it advances must clearly be reckoned with by every philosophic student of the human mind ; and I doubt if they have ever, within comparable limits of space, received so persuasive a formulation.

C. A. CAMPBELL.

La Philosophie Anglaise Classique. By EMMANUEL LEROUX and ANDRÉ LEROY. Paris : Librarie Armaud Colin, 1951. Pp. 214.

THIS book, in view of its comprehensive scope and concise and, for the most part, accurate presentation, is without parallel among English histories of philosophy. The first six chapters, covering the period from the early medievals to Berkeley, was written by Professor Leroux, who was prevented by an untimely death from finishing the work. The manuscript was preserved by his friend M. Georges Davy, and the work was completed for publication by Professor Leroy, who added five chapters covering the period from the early eighteenth century British moralists to John Stuart Mill, including chapters on the associationists and early economists.

The erudition of both authors is apparent beyond question, and the comparatively brief passages of criticism are for the most part sage. The reader will, however, be aware of a difference between their attitudes to the philosophical performances of the strange inhabitants of the island fringe of Europe. Professor Leroux was plainly fascinated by and, in some cases, enamoured of them. He studies them with zest and usually gets right inside the skin of their thoughts. Bacon and Berkeley seem to be his favourites ; he finds some difficulty in understanding the high reputation of Locke. Professor Leroy takes a more detached and businesslike attitude. For him English philosophy is a phenomenon to be conscientiously studied and accounted for. His account is given in the conclusion. Classical English philosophy is " *une philosophie de la raison activement engagée dans les problêmes de l'existence humaine* ". It is characterised by a " *goût du concret* ", and it finds the concrete principally in " *l'expérience mentale* ", " *l'expérience vécu* ". This predilection for the concrete restrains English philosophers from the construction of metaphysical systems, gives them a preference for experimental rather than logical reasoning, and, aided by the inner moderation of emotion and outer restraint of expression traditional in the country, results in a characteristic " reasonableness ", which is distinct from " rationality ". The " reasonable " philosopher, according to Professor Leroy, is engaged in a " *lutte mobile* " with the given, which he seeks to parcel and master " *sans le violenter* ". The resulting philosophy is " without doubt insufficiently systematic " ; for the regularity of coherent systems comes from sources far removed from " *l'expérience vécu* ". What these sources are, and how they should be used we are not told. Is it more formal logic, a less Bible-ridden religious consciousness, exalted oftener to a " *ravissement purement affectif* ", or a dash of Bergsonian intuition that English philosophy lacks ? There are hints of all three suggestions, but not more than hints.

Turning back to the main body of the book, the summaries of the medieval philosophers are commendably brief and lucid. A comparison with the article on Duns Scotus in the *Encyclopaedia Britannica* will show the reader how mercifully he has been treated. An excellent outline of the doctrines of St. Thomas Aquinas is given as a prelude to the account of the reactions of Roger Bacon, Duns Scotus and William of Occam.

Francis Bacon is the first philosopher who really lights Professor Leroux's tinder. Bacon is presented as a personality worthy of wonder and interest, unscrupulously ambitious in political life, as a thinker ambitious for the improvement of humanity. His impatience both with the authority of the ancients and with the haphazard empirical discoveries of the moderns, and his determination to provide a scientific method which would make the discovery of principles as rapid and fruitful as their application by the mechanical arts, are well described.

There follows a brilliant summary and criticism of Bacon's conception of science and scientific method. The reader is shown how the methods of elimination, induction, and step-by-step generalisation depend on a grand-scale metaphysic of efficient causes, according to which all the phenomena of nature arise from the combinations of a small number of " forms " or " simple natures " conceived as laws of movement and structure. Only if such forms are presupposed can we hope by constructing " tables of presence and absence " to discover the form to which a given phenomenon is due. The problem of induction is " magisterially posed " but incompletely solved (p. 40).

For if *a priori* presuppositions are once allowed how are we to distinguish them from the " idols " which Bacon is out to exorcise ? By their utility perhaps ? Professor Leroux suggests that Bacon is a bit of a pragmatist, quoting " what is most useful in the order of practice is most true in the order of knowledge " (p. 28). Or by some rationalistic procedure ? Such would be needed in any case to justify Bacon's admission of an order of final causes and sublime morality behind but distinct from the order of efficient causes and human morality, which can be empirically investigated. A combination of rationalism and pragmatism would fit well with the recommended model of the bee, opposed to the pure rationalism of the spider and the pure empiricism of the ant.

Professor Leroux's study of Hobbes is equally concise and illuminating. Whereas Bacon founded an incomplete system on truths, our knowledge of which he was unable to explain, it was the genius of Hobbes to build " robust constructions on partial truths " (p. 59). " His system is the most remarkable and coherent example of radical materialism in modern, and perhaps in all, times " (p. 58). This logical coherence of Hobbes endears him to the French Professor in spite of his absurdities, and so does a kinship of purpose between Hobbes and Descartes, though the latter is credited with greater wing-span (*envergure*) (p. 44). Descartes set out to reconcile modern science with the doctrines of the Church, and effected it by a dualism of spirits and machines, the former the province of religion, the latter of science. Hobbes set out to reconcile modern science with the authority of sovereigns, and effected it by a monism of machines, in which obedience to sovereigns was a necessary mechanical device. Some indulgence for Hobbes' naïveté is implicitly claimed for a philosopher who did not read the *Elements* of Euclid till he was forty (p. 44), and became an enamoured convert to deductive method, to the extent of ignoring such inconvenient empirical facts as the obvious disadvantages of absolute power (p. 59).

The chapter on Locke is derogatorily entitled " *Locke, l'initiateur de la procédure empirique, ou l'incohérence profonde* ". Nevertheless, Locke's position in the history of philosophy and the reasons for his stimulating influence on his successors are justly described. Neither Bacon nor Hobbes founded a school ; the former devised for science a methodology which she was well able to work out for herself, and Hobbes devised a foundation

for a political absolutism which his compatriots did not want. Locke on the
other hand initiated the Theory of Knowledge as a subject distinct from
natural science, theology and metaphysics, and devised a foundation for
responsible government and religious toleration, which nis compatriots
did want. The blow, " rude, perhaps mortal ", which Locke gave to the
traditional notion of substance, and his arguments for the inaccessibility
of real essences and necessary connexions freed science from the pretensions
of *a priori* demonstration, and placed it under the genial rule of probability,
whose precise nature he left to his successors to determine. His incoherences
themselves were " profound " not merely in the sense that they were
glaring, but in that they brought into focus the deeper problems of epis-
temology and ethics, particularly how we get from ideas, conceived à la
Descartes, to knowledge of real things, and the relations between rational,
revealed and utilitarian morality.

But Professor Leroux remains puzzled that so important a role was
played by a philosopher of so little *éclat*, so little vigour (!), so little *accent*,
no *élan*, no sense of mystery or of the hierarchy of values, a man who
includes health and eternal blessedness in the same list of durable pleasures.
I suspect also that the author does not think that Locke's incoherences
can ever be resolved by an epistemology which altogether eschews meta-
physics ; which is perhaps partly why he prefers Berkeley.

The chapter on Berkeley opens with a skilful piece of stage-setting.
The advance of natural science, despite the piety and caution of Newton,
had aroused acute religious controversy. Abusing the premisses of Newton
and Locke, free-thinkers like John Toland and Antony Collins had put
forward a dogmatic materialism. Samuel Clarke countered with an un-
original defence of traditional rationalism, and Malebranche with a novel
version of the Cartesian position. Newton's rejection of gravitation as a
true efficient cause, his slogan " hypotheses non fingo ", quoted self-
explanatorily in its context, and, barely consistent with the latter, his
concept of absolute space are rightly included in the scenery.

The figure of Berkeley is swiftly erected, standing truly on its two feet,
the analysis of " existence " and the rejection of abstract ideas. Con-
sidering the latter, Professor Leroux sees that it is not a personal defect
of imagination but the self-contradictoriness of the alleged conceptual
feat that Berkeley is emphasising, and that his second edition concession
of the ability to attend to some features of an object to the exclusion of
others is no retractation of his denial of our ability to conceive as existing
separately attributes whose separation implies a contradiction.

The analysis of the arguments of the *Principles* and the *Three Dialogues*
is clear and accurate and illuminated by well-chosen quotations from
the *Commonplace Book*. In connexion with Berkeley's account of
natural science the *De Motu* and the *Alciphron* are also considered.
Though the philosophical import of the latter is first said to be " mince "
(p. 108), the Professor later devotes more than a page to the theory of
signs and their importance to science, elaborated in the seventh dialogue,
and suggests (p. 113) that Berkeley has here slipped from his original
advocacy of the consideration of " naked ideas " to a pragmatism of
useful notations. This is questionable ; the method of progress by
appropriate choice and skilful management of signs is surely advocated
solely for mathematics and science, and neither recommended nor em-
ployed by Berkeley in philosophy.

The *Siris* is well interpreted ; the rehabilitation of Platonic ideas is
rightly seen not as a recantation of the denial of abstract ideas, but as an

identification of " la connaissance intellectuelle " with the " connaissance réflexive " which spirit enjoys of itself (p. 115). Professor Leroux does however detect a contrast between the robust and loving defence of the reality of sensible things in the *Principles*, and the description of them as " fleeting phantoms " in the *Siris* (p. 116).

The final assessment of Berkeley's philosophy omits any criticism of his phenomenalistic account of matter, and laments that it was this aspect of his work which most impressed his English successors. The assessment emphasises rather the " *Spiritualisme concret ou réflexive, qui devait trouver son véritable développement chez un Maine de Biran, un Ravisson, et surtout un Bergson* ".

Professor Leroy's first chapter, " *à la recherche d'une morale efficace,*" calls for little comment. It is a businesslike survey in twenty-five pages of British moralists from Shaftesbury to Robert Owen. Some authors are merely mentioned without discussion, for instance the bulky Abraham Tucker, whose pages the author admits he has only turned over at random.

The chapter on Hume is a different matter. It is rather surprisingly entitled " *David Hume, le philosophe de la spontanéité* ". The text elucidates the title. For Professor Leroy Hume is important, not for his negative and sceptical conclusions, but for his empirical report of the behaviour of the mind and its constituent impressions and ideas. The latter are not passively registered and preserved, like photographs on a cinematograph film, but live and blend and unite and attract one another in a manner all their own. Association, custom, fusion, the formation of general ideas, the interaction of feelings and ideas, the reflex operation of general rules and the fusion between them and the original associative tendencies, all these are examples of " spontanéité ". Similarly in Hume's moral theory we find reflex attitudes moderating the primary movements of sympathy and human inventiveness devising artificial systems on which artificial virtues are founded. From all this results a picture of man's mind as having an inventive and adventurous character " *qui s'acharne a projeter sur le donné ses propres schèmes ; d'ailleurs, a son insu, cequi lui fait accorder a ses propres fantômes une réalité ' objective ', dont il est dupe aux heures, si fréquentes, où il ne se garde pas contre eux* "(p. 176)

One feels that a false twist has been given to Hume's philosophy. There are, according to Hume, no hours, however infrequent, in which we can guard ourselves against our natural beliefs. Such attempts lead only to brief attacks of sceptical paralysis. Nor in our normal hours are we " dupes " of anything, unless we are dupes of prejudice, unregulated fancy or passion, superstition or education. Hume's own account of the mind's behaviour must be a product of empirical reasoning, and cannot therefore be used to invalidate that procedure, whatever contradictions and mysteries it leaves unsolved. To invalidate it we need a picture of the mind derived from some other source, such as the transcendental arguments of Kant. Such arguments Hume would not accept.

Professor Leroy's predominant interest in Hume's psychological reporting of the interplay of mental forces leads him to ignore one fundamental point in Hume's philosophy, the capital distinction between relations of ideas and matters of fact. Professor Leroy indeed seems to use the expression " *relations d'idées* " indiscriminately to cover both the logical and mathematical relations for which Hume ear-marked it, and the associative relations of resemblance, contiguity and customary connexion. By this one omission he robs Hume's philosophy of its principal contribution to epistemology, the rigorous distinction between logic and fact.

Despite this defect, the chapter is full of lucid and concise analyses of Hume's arguments, particularly those concerning the external world, the self, liberty and necessity, and morals.

The remaining chapters on the early systems of political economy, associationism and intuitionism, and the brief chapter on John Stuart Mill share the informative and scholarly conciseness which is characteristic of the whole book.

D. MACNABB.

David Hume : His Theory of Knowledge and Morality. By D. G. C. MACNABB. London : Hutchinson's University Library, 1951. Pp. 208. 7s. 6d.
Hume : Theory of Knowledge. Edited by D. C. YALDEN-THOMSON. Edinburgh : Nelson, 1951. Pp. xxvii + 265. 7s. 6d.
Hume : Theory of Politics. Edited by FREDERICK WATKINS. Edinburgh: Nelson, 1951. Pp. xxx + 246. 7s. 6d.

Mr. MACNABB's book is expressly aimed at those " who are not principally or professionally engaged in the study of philosophy ", in order to explain to them why.Hume receives so much attention from philosophers. Having within the limits of two hundred pages taken on both the epistemology and the ethics of Hume, he cannot afford to deviate either far or often from the main routes, nor to do much more than map them on a fairly small scale. This he does with great clarity, good sense, and wit, and without a trace either of the critic's condescension or of the acolyte's piety.

Having distinguished between Hume's use of the Experimental Method, which is a method of proof, showing, *e.g.* how we in fact form our ideas, and his use of the Method of Challenge, which is a method of persuasion (*e.g.* show me an inference from observed to unobserved which is not causal; or, in regard to the allegedly demonstrable moral relation of fitness " to this I know not what to reply, till someone be so good as to point out to me this new relation "), MacNabb proceeds to Part I of his book, which is concerned with Hume on the Understanding, and which takes up 150 pages ; the remaining 50 pages, comprising Part II, deal with Hume's account of Morals.

In the nine chapters of Part I all the main topics are succinctly run through. A warning against Hume's mistake in psychologising his doctrine of impressions and ideas is one that any beginner (and not only beginners) would profitably heed. In the chapter on Abstract Ideas a distinction is clearly drawn between Hume's nominalism and Berkeley's, although it might be argued that the distinction is slightly unfair to Berkeley. Next follow five chapters in which Hume's main contributions to empiricism are expounded : knowledge, belief, probability, causality, and necessary connexion ; and this part is concluded by one chapter on our belief in the existence of material objects, and by one on the nature of mind and of personal identity. The lucidity and fairness with which MacNabb expounds Hume's views and with which he replies to objections may contain a certain danger for the less wary student : he might well be persuaded by the clarity of MacNabb's arguments as far as they go into thinking that there is no further to go. If there is a general criticism to be made of MacNabb, it lies in this direction : that he encourages the reader to believe too often that Hume has said the last word. On cause, for instance. Is it so clear just what question Hume was trying to answer ? And if we

take it to be the question that MacNabb does ("What do we *mean* by saying that flame and heat are necessarily connected ? "), is it so clear that the three objections to Hume's answer which MacNabb considers and rejects are all there are ? For instance, is it certain that Hume would not be committed to saying, contrary to his official doctrine, that we *can* know the truth of general causal propositions ?

In Part II the theory of morals is as fully outlined as is possible within such narrow confines, and is given a sympathetic statement. MacNabb criticises Hume for restricting ' reason ' over-narrowly, so as to exclude from its scope the tendency to judge impartially, and he proposes to amend the theory by substituting ' attitude' for 'feeling', his reasons being nowadays familiar. As he hints on the final page, this change raises at once the question whether the difference between a so-called attitude of approval and a belief is as clear as some subjectivists (and their opponents) have supposed.

The two Nelson volumes are the first in a new series of philosophical texts edited for the use of university students. Professor Watkins' contribution should be particularly welcome, as it brings within the same covers Book III, Parts I and II of the *Treatise*, and a wide selection of the *Essays*, including " That Politics may be reduced to a Science ", " Of the Original Contract ", and " Idea of a Perfect Commonwealth ". In addition to the editor's introduction, which contains a lucid exposition of Hume's views on natural law, the empirical basis of political norms, and the distinction between reason and the calm passions, the volume includes in an appendix a selection of variants on the text of the *Treatise* (first edition) which have been discovered by Professor Klibansky and which he believes to have had as their author Hume himself.

Mr. Yalden-Thomson's volume consists of the *Enquiry Concerning Human Understanding*, a selection of those topics from the *Treatise*, Book I, which play little or no part in the *Enquiry*, and the *Abstract*. A brief introduction is followed by a useful working bibliography to which in any future edition MacNabb's book will certainly need to be added.

A. D. Woozley.

Plato's *Charmides*. By T. G. Tuckey. Cambridge University Press, 1951 (Cambridge Classical Studies). 12s. 6d. net.

This study of the *Charmides* is the work of a young man who, after being elected to a Bye-Fellowship in 1939, joined the Royal Signals, and was killed at Cassino in May, 1944.

The work is almost entirely devoted to the principal crux of the dialogue, namely the passage from 165 c. to the end, in which the possibility of self-knowledge is considered. Starting from the proposal to define σωφροσύνη as knowledge of one's self in the sense of the proverbial γνῶθι σεαυτόν, the disputants pass to what seems quite a different topic, *viz.* the idea of a science whose sole business it is to examine the nature of knowledge and ignorance ; an ἐπιστήμη ἐπιστήμης or ἐπιστήμη ἣ αὐτὴ αὑτὴν γιγνώσκει. It is a difficult passage which, as Mr. Tuckey says, ' has been very variously interpreted, and its relevance to the rest of the argument very variously assessed '. His own opinion appears to some extent from the beginning. ' It is probable that commentators have themselves needlessly increased the difficulties and have sown confusion by introducing conceptions derived either from post-Platonic philosophy or from Plato's own philosophy at a

later stage of its development'. After a detailed study of the argument during which the views of many scholars are sympathetically stated, Mr. Tuckey comes to the conclusion that the principal reason why Plato fastens upon the topic of self-knowledge is that it is one which was inevitably suggested to his mind by the personality of Socrates, who did claim to possess the knowledge of his own ignorance and of that of others. This view also makes clear the relevance of the discussion to the main problem of the dialogue, since σωφροσύνη was the characteristic virtue of Socrates, His personality links together two kinds of self-knowledge between which there is no *logical* connection.

This seems to me a more probable explanation of the form of the dialogue than any of those which are here passed in review ; but there is perhaps more to be said in favour of Taylor's analysis of the passage than Mr. Tuckey allows—he seems to be unduly frightened by the word ' epistemology'. Various questions which arise incidentally are well discussed—whether Plato has some contemporary doctrine in view, to what extent he is conscious of the fallacies in his argument, and so forth. The work would doubtless have been pruned in revision, but as it stands, it will be of great service to anyone who in future attempts the study of this dialogue.

<div align="right">D. J. ALLAN.</div>

The Moral Life and the Ethical Life. By ELISEO VIVAS. Chicago: University Press. 1950. Pp. xix + 390.

THIS book is divided almost equally into an attack on a number of theories now current in America and a positive account of ' the Moral Process '. It is also written in two quite different styles and tempers ; and this is a pity, since the style of Mr. Vivas' criticism is unworthy of that of his construction. He is a recent convert from a Deweyan brand of naturalism and, like all converts, he is more than usually impatient of those who will not see the light. He lays about him with a petulant gusto that can only serve to bias his readers in favour of his victims and his misrepresentations of his opponents' views are sometimes so gross that they cannot be attributed to mere inadvertency. James talks ' vicious nonsense ', Dewey talks ' monstrous nonsense ', and the following quotation is typical : "I do not believe that positivists, hard-headed empiricists, realistic moralists, Machiavellians, Hobbesians, Nietzscheans and the rest of the numerous descendants of Thrasymachus could have read thus far ; but, if they have, they are now requested to throw away the book and put their time to better use, since they can get out of the following pages nothing but dull weariness and irritation."

Mr. Vivas' earlier pages teem with ' isms ' : hedonism, subjectivism, empiricism, naturalism, humanism, secularism, pragmatism, cultural relativism, positivism, scientificism. All are derived from and, apparently, have not advanced one step beyond the position taken up by Thrasymachus, a position that Plato himself disdained to take seriously. All are accused of preaching that Might is Right and that men either are or ought to be cruel and selfish. There are, he says, criteria of perfection by which all societies can be criticised. " If this is nonsense, we would do well to quit fooling ourselves with unstable attempts to look at the truth, for the other firm alternative is the frankly might-makes-right morality of the positivists, which Stevenson clarified for us in our day and which we can trace back through William James, through Hobbes, to Thrasymachus."

But not only do none of the authors condemned say that might makes right ; there is no reason why they should.   Mr. Vivas makes frequent use of Hume's argument that moral judgments do not follow from statements of fact or from logical analyses ; but at the same time he insists that his opponents, when they refuse to fall into this trap, ought to do so.   But the doctrine that moral valuations are logically posterior to human interests, that good and bad can be defined in terms of pleasure or desire, cannot imply any theory whatsoever as to the nature of human interests.   Hobbes held the extreme position that " whatsoever is the object of any man's appetite or desire, that is it which he for his part calleth good ".   But, if fighting and selfishness are ' human interests ', herding and altruism are so too ; and Hobbes himself lists, among our other desires, " Desire of Good to another, Benevolence, Good Will, Charity ".

In contrast to the philosophers criticised Mr. Vivas pins his faith to a theory of the ' ontic objectivity of values '.   This is established by first showing that values are ' phenomenally objective ' (it is a question of fact whether X seems good to Y or not) and then denying the " bifurcation of nature into two distinct classes of objects ".   But surely this proves too much.   And the real objection to Mr. Vivas' doctrine is that this notion of objective values is introduced as an explanation and proof of the doctrine that we ought to value certain things ; and this, on Mr. Vivas' own showing it cannot be.   These ' values ' cannot bridge the awkward gap between ' ought ' and ' is ' ; they merely serve thinly to disguise the fatal step.   No doubt, in ordinary English, ' charity is a value ' is just a barbarism for ' People ought to be charitable ' ; but for Mr. Vivas it must play a different role.   It must be a statement about a non-natural realm of values and also one from which ' People ought to be charitable ' somehow follows.

The use of the word ' values ' as a noun is a pernicious barbarism for which Mr. Vivas is not wholly to be blamed, as it is common form in contemporary American philosophy.   But rarely is it used to perpetrate such sophistries as this (abridged) proof that hospitality is an objective value.   ' Hospitality is a value.   Hospitality objectively resides in the people and actions called hospitable.   Therefore at least one value objectively resides.'   But in plain English this becomes something like : " Hospitality is valuable ; and the question whether X is hospitable or not is a question of fact, amenable to observation, on all fours with the question ' Does iron rust ? '."   Clearly nothing about the logical status of moral judgments follows from these two premises ; the introduction of the noun ' values ' simply gives a syllogistic elegance to a plain *non-sequitur.*

Mr. Vivas has some interesting things to say about the genesis of moral indignation ; but his manner of posing the problem leads him into insoluble mysteries.   Moral indignation cannot, he says, be the outcome of repression, as the psychologists say, because it is a new quality, phenomenon or entity, radically different from ' purely physical, amoral attitudes '.   We must postulate a special moral faculty which is there from the start. But does the fact that intelligent speech differs from babbling require the postulation, in babies, of a special latent faculty of speech ?   And does this mean any more than that babies, unlike giraffes and motor-cars, are, at birth, such that in time they will be able to speak intelligently ?

If we ask what Mr. Vivas means by ' morality ', ' moral force ', ' moral condemnation ', we find, I think, a subtle but disastrous perversion of the argument against the naturalistic fallacy.   Clearly ' moral ' is here contrasted with ' non-moral ' not with ' immoral "; so one thing that might be

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meant by 'moral force' is some motive that we would be prepared to call good or bad, such as love or hatred, as opposed to an interest in conchology or philately. But there is presumably no mystery about the genesis of these 'forces', which makes them opaque to ordinary scientific methods of enquiry. Evidently what Mr. Vivas means by a 'moral force' is not some force which is either good or bad but a force of a special kind, of which goodness' or 'badness' or 'sense of duty' or 'sense of guilt' might be names. A moral conflict is for him not a conflict between two forces, one good and one bad; it is a conflict in which one of the participants is moral goodness (or badness) itself. Moore's doctrine that 'goodness' is not to be identified with any natural quality or object and his innocent tautology that only good things are good is converted by Mr. Vivas into the type-fallacy that only moral goodness itself is good. Beauty itself, Αὐτὸ τὸ καλόν, is alone beautiful; truth alone is true.

The same fallacy underlies Mr. Vivas' attempt to distinguish sharply between moral and non-moral values. Pleasure, prestige and comfort are all, of course, values. But, where Moore hammers away at the point that none of these can be identical with goodness and that to call any of them 'good' must be to say something non-tautologous, Mr. Vivas succumbs to the not uncommon temptation of thinking that these cannot be morally good. However long we make the list of values, we do not light on anything that is morally valuable until we mention 'moral virtue' itself. To say that something is morally valuable is to say that it ought to be espoused; but in the end we find that moral virtue is the only thing of which it is true to say that it ought to be espoused.

In Parts II and III Mr. Vivas sets himself to the task of vindicating the objectivity, the 'requiredness', of values without invoking an infallible conscience; and here he struggles with the difficulties with honesty and insight. His problem is to combine the subjective principle that a choice, to be moral, must commend itself to me and not be an alien *diktat*, with the objective principle that values require my allegiance. "Succinctly stated", he says, "a moral perplexity is resolved by searching for a solution which sustains and strengthens or at least does not positively threaten or does not threaten too radically the values which we take to be constitutive of our personality or natural self. . . . For the moral philosopher, the human personality (man at the moral level) is centrally constituted by a hierarchically organized system of values, some of which he 'espouses', some of which he merely recognizes, but not all of which are normally discovered by his reflective consciousness as constitutive of himself."

It is not surprising that Mr. Vivas finds in injustice the core of all moral evil. "Why does one—no, why do I—disregard the person of another? Why indeed, if not because I fear that his plea will threaten my cherished espousals?" And this fear can only be allayed if I place at the centre of my own cherished espousals respect for the person of 'the other'. We have come by a long, I think a needlessly long, route to a moral principle that is not only very familiar but which would be accepted (though in different terminology) by almost all the philosophers whose corpses litter the wayside. But are we any nearer the solution of a moral perplexity? The command 'Respect the person of the other' invites the question: 'Which other?' and here Mr. Vivas gives us no guidance. To illustrate this let us consider his remarks on regicide.

"You can, if you are strong enough, dethrone the king; you can tie him to your chariot and drag him along the crowded streets. As a man you cannot dethrone another man and drag him without dethroning and

dragging yourself first. To kill a king may be a *political* necessity which a Lenin can argue about with a Suarez. It can never be an ethical necessity, since the only necessity that the ethical man would recognize is that of living according to his primary insight. When the two necessities—the political and the ethical—clash, you may be facing a tragic conflict in the sense that whichever way you choose to resolve it involves irreparable loss. But your ethical insight will tell you that the greatest loss of all is that which is involved in repudiating your destiny." But what if the king is a monster of iniquity ? What if to spare him is to connive at the death of his innumerable victims ? What, in this case, is my destiny ? And how does my primary insight tell me what to do ? I think the implication is that I must at all costs keep myself Simon-pure. After all, if I kill the king, I do murder. If I spare him, he does murder, but not I. So the final outcome of ontically objective values and respect for others is a system in which my sole duty is to wrap myself in the cloak of my moral integrity, and let the devil take the rest if he chooses. It is their funeral and his, not mine.

<div align="right">P. H. NOWELL-SMITH.</div>

Man's Freedom. By PAUL WEISS. Yale University Press (London : Geoffrey Cumberlege). 1950. Pp. ix + 325. £1 12s. 6d.

PROFESSOR WEISS gives the metaphysical ground for man's freedom, and then proceeds to show how this freedom is exemplified in various spheres of human activity.

Man's nature, like that of everything else, terminates in a future which is genuinely possible, being determined only in its general character but undetermined in its concrete particularity. Man "internalizes" this future and thus is able creatively to determine both his own future and that of those things with which he interacts.

All things have some value, and they have a correlative right to have this value preserved or enhanced. But all human activity involves the use of some things as means to an end desired for itself. Hence no one can avoid some diminution of value, and therefore doing some injustice, whenever he acts. But men ought to seek to bring about a state of affairs in which the value of every individual thing is "maximized" consistently and harmoniously with that of all other things. Happiness, therefore, cannot be the supreme good, for it is concerned only with human values, and there are other things besides men.

Aside from the general thesis, there is much of interest in incidental discussions. Analysis of traditional concepts nearly always reveals some novel aspect which is suggestive and, for some at least, is likely to be provocative to a degree. Not infrequently the result of analysis has the appearance of paradox (*e.g.* not only does *ought* imply *can*, but *can* also implies *ought*), but this appearance often resolves into a truism when one remembers the metaphysical presuppositions underlying the analysis.

There is a regrettable tendency to logorrhoea, and the author's predilection for the adverbial use of " though " distracted at least one reader. The book ends with a recapitulation of the argument which falls neatly into ninety-nine headings, and an index.

I have noted the following misprints : p. 45, l. 13, for " other " read "others "; p. 82, l. 26, for " live " read " life "; p. 150, l. 27, for " points " read " point "; p. 167, l. 3, for " overly " read " overtly ".

<div align="right">L. E. THOMAS.</div>

The Indian Philosophical Congress. Silver Jubilee Commemoration Volume.
Ed. Dr. T. M. P. MAHADEVAN. Madras, 1950. Pp. viii + 311.*

THE volume includes the addresses given at the jubilee meetings of the
congress in December 1950, other than the presidential address by Sir S.
Radakrishnan and the addresses of the foreign delegates, which will be
published separately. Thanks to the generosity of the Philosophical
Congress it was my great privilege to attend these meetings and also to
engage in an extensive lecture tour of Indian universities. The impression
on my mind was one of very much philosophical activity and interest.
The number of philosophical students, even specialized students, and
teachers at most Indian universities is very great and so is their enthusiasm.
The audience at the congress meetings amounted to about 700, a figure
which it would be hard indeed to equal at a philosophical function in the
West. Philosophy in India, however, runs on rather different lines from
those common in this country to-day, though personally it struck me as
much less exotic, much less difficult to grasp owing to difference of back-
ground, than much of French and German philosophy to-day. The differ-
ences between it and the type of philosophy most prominent in Britain and
U.S.A. at the present time may be summed up by saying that it is more
in contact with religion and with practical aims (practical not in the sense
of making money but in the sense in which the search for the highest good
is practical) and less in contact with science. Partly because of this diver-
gence of interest I found that the most common approach is still that of
metaphysical idealism, which we have got into the habit of looking on as
dead. No form of modern positivism has made a wide impression, and
the philosophical atmosphere is more like I imagine that of the nineties at
Oxford to have been than what we enjoy in England to-day. (Except
on a very superficial theory of inevitable progress in philosophy this is,
of course, not necessarily a reproach to India, tho' no doubt her thinkers
would benefit somewhat by a greater familiarity with and a greater
readiness to apply the methods of modern analysis.) Studies of logic and
linguistics of the more modern type find no place in the proceedings of the
Congress and there is no talk about the analysis of common-sense pro-
positions. It would be wrong to conclude that Indian metaphysicians
cannot effectively make subtle philosophical distinctions—they certainly
do so—or that there is necessarily no " verification " available for their
philosophical theories. Their claim is usually, I think, that anyone can
verify them for himself by mystical experience if he is prepared to go
through the long and hard training necessary, and that they can give
support for them in analysis and argument to make them at least reasonable
opinions for those who have not attained that stage of verification.

To turn to the specific proceedings of the Congress, the inaugural
address of the president Sir S. Radakrishnan, which will appear in a second
volume, was a most eloquent and inspiring account of the function of
philosophy with an éye specially on practice and on the emergence of the
world from its present pitiable condition of international discord, and the
Indian Press over-optimistically held out hopes that the assembled philo-
sophers might make suggestions which would alleviate the present inter-
national crisis. I myself agree with Professor Price that, since philosophy
takes a long time to exercise its influence, present philosophers are more
likely to contribute *quâ* philosophers to the solution of a crisis thirty years

* Copies available with Prof. N. A. Nikam, Secretary, Indian Philosophical
Congress, Basavangudi, Bangalore, India. Rs. 20/-

hence than to the solution of a present one; and though there was an interesting symposium on the place of the philosopher in modern society, very little was said directly about the concepts and controversies of politics, but the Indian addresses throughout the Congress were practical in the sense of consciously striving to pave the way for a philosophical outlook which, if it could only be adopted generally, would transform the international situation by transforming the viewpoint of the individual. The quality of the papers was very good, but they were in general too short to be more than mere outline programmes, though very well worth reading as such. The limitation in size was, of course, inevitable at such a conference, and the Indian philosophers hospitably made it worse for themselves by allowing a far more liberal ration of the limited time to the foreign delegates, whose papers will be printed later. In the present volume I should like to mention specially the paper by Professor A. C. Mukerji on "Traditional Epistemology" and that by Professor J. N. Chubb on "The Nature of Truth" in which he commends a theory of truth that eludes the ordinary classification. The all too short paper by Professor D. M. Datta on "Symbolism in Religion" should interest both the religious and the linguistic; and as far as one who is not familiar with the original authors discussed can judge, the symposium on Sri Aurobindo seemed on the whole very good. It dealt with the important and much debated question whether Indian philosophy is constrained to reduce the physical world to mere illusion. A wider popular appeal would be possessed by the symposium on "The Fundamentals of Living Faiths", and it was very good to find hardly a breath of intolerance in the discussion. The book closes with thirty pages of messages of greeting from numerous persons and institutions and a reprint of the finely poetic address by Rabindranath Tagore given at the first session of Congress in 1925 showing how deeply a philosophy is rooted in India even in the thought of the common man.

<div style="text-align: right">A. C. EWING.</div>

Received also :—

Aristotle: The Nicomachean Ethics. A Commentary by the late H. H. Joachim. Ed. D. A. Rees. Oxford, at the Clarendon Press (Geoffrey Cumberlege), 1951, pp. vi + 304, £1 5s.

Aristotle's 'De Anima', with the Commentary of St. Thomas Aquinas. Translated by Father K. Foster and Father S. Humphries. London, Routlege & Kegan Paul Ltd., 1951, pp. 504, £2 2s.

Bibliographia Philosophica, 1934-1945. Ed. G. A. De Brie. Vol. I: Bibliographia Historiæ Philosophiae. Brussells, Editiones Spectrum, 1950, 45 Dutch florins.

E. S. Brightman, *An Introduction to Philosophy.* Revised edition. New York, Henry Holt & Co., 1951, pp. xvii + 349.

G. B. Burch, *Early Mediaeval Philosophy.* Kings Crown Press (London : Geoffrey Cumberlege), 1951, pp. viii + 142, 14s. 6d.

R. Carnap, *Logical Foundations of Probability.* The University of Chicago Press, 1951, pp. xvii + 607, $8.50.

M. H. Carré, *Does it Follow?* Practice in Logical Thinking. London, Thomas Nelson & Sons Ltd. (Charter for Youth Series), 1951, pp. xiv + 174, 5s.

Mario Corsi, *Le Origini del Pensiero di Benedetto Croce.* Firenze, La Nuova Italia, 1951, pp. 183.

A. Darbon, *Une Doctrine de l'Infini*. Paris, Presses Universitaires de France, 1951, pp. 250, 600 frs.

S. Daval, *Philosophie Générale*. Paris, Presses Universitaires de France, 1951, pp. viii + 443, 680 frs.

S. Daval and B. Guillemain, *Philosophie des Sciences*. Paris, Presses Universitaires de France, 1950, pp. xxxii + 563, 740 frs.

Descartes su mundo moral y religioso by F. G. Ríos. Buenos Aires, Ano del Libertador General San Martin, 1950, pp. 50.

Dictionary. *Diccionario de Filosofia* by J. F. Mora, 3rd edn. Buenos Aires, Editorial Sudamericana, 1951, pp. 1047, $150.

J. Dopp, *Leçons de Logique Formelle*, Pt. I : *Logique Ancienne*, Pts. II and III : *Logique Moderne*, I and II. Louvain, Editions de l'Institut Supérieur de Philosophie, 1950, pp. xi + 166, xi + 216, xvi + 274.

Evolutionary Thought in America. Ed. Stow Persons. Yale University Press (London : Geoffrey Cumberlege), 1951, pp. x + 462, £1 12s. 6d.

Fenomenologia e Sociologia by E. Castelli, U. Spirito, G. Bontadini, U. Padovani, P. Carcano, E. Opocher, E. Severino, M. de Corte, G. Gusdorf, A. Dempf, J. Eymard. Padova, Editions Liviani, 1951, pp. 144, L. 700.

B. Guillemain, *Philosophie Morale*. Paris, Presses Universitaires de France, 1951, pp. vii + 569, 860 frs.

Kant, *Deutung und Darstellung der Theoretischen Philosophie Kants* by Felix Grayeff. Hamburg, Richard Meiner Verlag, 1951, pp. xxiii + 225.

Immanuel Kant by G. Martin. Köln, Kölner Universitäts Verlag, 1951, pp. 244.

J. H. M. M. Leonen, *De 'nous' in het Systeem van Plato's Philosophie*. Amsterdam, Jasonpers Universiteitspers, 1951, pp. viii + 297.

G. Marcel, *Homo Viator*. London, Victor Gollancz, 1951, pp. 270, 16s.

J. Maritain, *Philosophy of Nature*, New York, Philosophical Library, 1951, pp. x + 198, £3.00.

Tullio Mogno, *La Filosofia*. Firenze, La Nuova Italia, 1951, pp. 263.

N. R. Murphy, *The Interpretation of Plato's Republic*. Oxford, at the Clarendon Press, 1951, pp. 246, 18s.

Nietzsche. Philosopher, Psychologist, Antichrist, by W. A. Kaufmann. Princeton University Press (London : Geoffrey Cumberlege), 1951, pp. xi + 409, £2.

J. Paliard, *La Pensée et la Vie*. Recherche sur la Logique de la Perception. Paris, Presses Universitaires de France, 1951, pp. 314, 700 frs.

H. Reichenbach, *The Rise of Scientific Philosophy*. University of California Press (London : Cambridge University Press), 1951, pp. xi + 333, £1 8s.

E. J. Ryan, *The Role of the " Sensus Communis " in the Psychology of St. Thomas Aquinas*. Carthagena, Ohio, The Messenger Press, 1951, pp. ix + 198.

San Anselmo Proslogion. Texto y Traduccion. La Plata, Instituto de Filosofia, 1950, pp. 135.

G. Santayana, *Dominations and Powers*. Reflections on Liberty and Government. London, Constable & Co., 1951, pp. xv + 481, £2 2s.

F. G. J. Schelling, *La Esencia de la Libertad Humana*. Buenos Aires, Ano del Libertador General San Martin, 1950, pp. 152.

M.-F. Sciacca, *L'Existence de Dieu*. Paris, Aubier, Editions Montaigne, 1951, pp. 215.

A. E. Teale, *Kantian Ethics*. Oxford University Press (London : Geoffrey Cumberlege), 1951, pp. x + 328, £1 10s.

Tran-Duc-Thao, *Phenomenologie et Materialisme Dialectique*. Paris, Editions Minh-Tân, 1951, pp. 368, 650 frs.

J. A. Vazquez, *Ensayos Metafisicos*. Tucuman (Argentina), Instituto de Filosofia, 1951, pp. 95, 8 pesos.

J. J. Verne, *Combats contre la nuit*. Paris, Editions Colbert, 1951, pp. 256, 300 frs.

W. H. Walsh, *An Introduction to Philosophy of History*. London, Hutchinson's University Library (Philosophy), 1951, pp. 173, 7s. 6d.

G. H. von Wright, *A Treatise on Induction and Probability*. London, Routledge & Kegan Paul, 1951, pp. 310, £1 10s.

P. Wyser, *Der Thomismus*. Bern, A. Francke Ap. Verlag, 1951, pp. 120, S. Fr. 5.80.

F. H. Young, *The Philosophy of Henry James Sr.* New York, Bookman Associates, 1951, pp. xiv + 338. $4.50.

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Autobiography of a Schizophrenic. Bristol, printed by J. Baker & Son Ltd., 1951, pp. 64, 5s.

C. Berg, *The Unconscious Significance of Hair*. London, George Allen & Unwin Ltd., 1951, pp. 106, 15s.

C. Binger, *More about Psychiatry*. London, George Allen & Unwin Ltd., 1951, pp. xiii + 201, 10s. 6d.

D. Katz, *Gestalt Psychology* (Methuen's Manuals of Modern Psychology). London : Methuen & Co. Ltd., 1951, pp. x + 175, 12s 6d.

M. M. Moncrieff, *The Clairvoyant Theory of Perception*. London, Faber and Faber, Ltd., 1951, pp. 315, £1 1s.

Occupational Choice. An Approach to a General Theory, by E. Ginzberg, S. W. Ginzberg, S. Axelrad, J. L. Herma. Columbia University Press (London : Geoffrey Cumberlege) 1951, pp. viii + 271, £3 3s.

W. H. W. Sabine, *Second Sight in Daily Life*. London, George Allen & Unwin Ltd., 1951, pp. 208, 12s. 6d.

H. Schlötermann, *Vom Hellsehen*. Schlehdorf Obb., Bronnen-Verlag, 1951, pp. 79.

R. H. Thouless, *General and Social Psychology*. Third ed. revised. London, University Tutorial Press Ltd., 1951, pp. viii + 430, 17s. 6d.

———

F. Alexander, *Our Age of Unreason*. New, completely revised edition. New York, J. B. Lippincott Company, 1951, pp. 338, $4.50.

Aspects of Form. A Symposium on Form in Nature and Art. Edited by L. L. Whyte, London, Lund Humphries, 1951, pp. ix + 247, £1 1s.

J. Baillie, *Natural Science and the Spiritual Life*. Oxford University Press (Geoffrey Cumberlege), 1951, pp. 43, 5s.

Cartwrightiana : Elizabethan Non-Conformist Texts. Edited by A. Peel and L. H. Carlson. London, George Allen & Unwin Ltd. (for the Sir Halley Stewart Trust), 1951, pp. x + 268, £1 5s.

Sir Alan Gardiner, *The Theory of Speech and Language*. 2nd edn. Oxford, at the Clarendon Press (London : Geoffrey Cumberlege), 1951, pp. x + 348, 15s.

Handbook to Latin American Studies, 1947, No. 13. Prepared by the Hispanic Foundation of the Library of Congress, edited by F. Aguilera. Harvard University Press (London : Geoffrey Cumberlege), 1951, pp. x + 239, £1 5s.

T. Heywood, *How Smoke gets into the Air*. Published by the author, The Fortune Press, 1951, pp. 25, 5s.

The I Ching or Book of Chances. 2 vols. The Richard Wilhelm translation rendered into English by Cary F. Baynes. London, Routledge & Kegan Paul Ltd., 1951, pp. xlii + 395 and 371, £2 2s.

Magus, *Il Grande Vangelo*. Milan, Libreria, Editrice Universitaria, 1951, pp. 195.

R. Morris, *The Quality of Learning*. London, Methuen & Co., Ltd., 1951, pp. vi + 107, 7s 6d.

J. Seyppel, *Dekadenz oder Fortschritt*. Schlehdorf Obb., Bronnen-Verlag, 1951, pp. 64.

T. L. Vaswani, *A Prophet of the People*. Poona, Gita Publishing House, 1951, pp. 74, 4s.

T. L. Vaswani, *An Interpretation* by Dhan Raj. Hyderabad-Sind, Dhan Raj, 1936, pp. 39.

T. L. Vaswani, *Gita : Meditations*. Vol. I. Poona, Gita Publishing House, 1951, pp. 138, 10s. 6d.

A. Wenzl, *Unsterblichkeit*. Bern, A. Francke Ap. Verlag, 1951, pp. 213, S. Fr. 7.80.

Sir E. Whittaker, *History of the Theories of Aether and Electricity*. London, Thomas Nelson & Sons Ltd., 1951, pp. xiii + 434, £1 12s. 6d.

J. Z. Young, *Doubt and Certainty in Science* (B.B.C. Reith Lectures, 1950). Oxford, at the Clarendon Press (Geoffrey Cumberlege), 1951, pp. vi + 168, 7s. 6d.

A PROBLEM OF LEWIS CARROLL

Mind, July, 1951

CORRIGENDA

Page 394.   Premise no. 10.   *For* Q *read* $\overline{\text{Q}}$.

Premises no. 11 and 13.   *For* S *read* $\overline{\text{S}}$.

Page 395.   There should be a bar over every S, O and V, and over each Q up to and including line 18.

Page 395.   Line 30.   *Omit* P.

Page 396.   Line 2.   *For* S *read* $\overline{\text{S}}$.

Line 8.   *For* A *read* $\overline{\text{A}}$.

L. J. RUSSELL.

VIII.—NOTES

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